

VIVISECTION.

*Royal Society
for the
Prevention of Cruelty to Animals*



*and the
Royal Commission.*

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VIVISECTION

DIVISIONS.

Introduction—relating to the opinions and action of the Society.

Minutes of Mr. Colan's examination.

Documentary Evidence presented by the Society.

Other Evidence—Returns from Medical Schools—Epitomised witnesses' examinations.

Report of the Royal Commission.

Tabular Digest of Evidence.

VIVISECTION

The

Royal Society

for the

Prevention of Cruelty to Animals

and the

Royal Commission.

The progress of medical knowledge ought to be made compatible with the just requirements of humanity.—*Royal Commission.*

LONDON:

SMITH, ELDER & Co., 15, WATERLOO PLACE.

1876.

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INTRODUCTION.

THIS volume has a two-fold object—explanatory, as regards the proceedings and attitude of the Royal Society for the Prevention of Cruelty to Animals, in relation to the practice of subjecting living creatures to experiments for scientific purposes ; suggestive, as regards the nature and extent of such practice in the United Kingdom, the opinions of physiologists and medical men thereon, the evils likely to arise out of its unchecked abuses, and the consequent necessity of promoting a sound opinion on the subject. It is not a treatise, a learned exposition, a sensational disquisition, or a vehement philippic. To secure a dispassionate consideration of the subject, it has been necessary to restrict its pages to the data on which the Committee of the Society have formed their conclusions, and the opinions, necessarily epitomised, on which the Royal Commissioners have founded their recommendations. Persons who have leisure, inclination, and capacity of analysis, should study the Blue Book from end to end ; those who prefer a digest may find this volume useful, notwithstanding its imperfections ; in which event the Committee will have attained their purpose in directing the Secretary to prepare the following pages.

The Society consists of members holding at least six different views on this subject. There are vegetarians, who object to animal food and to animal destruction. Although they would probably destroy noxious animals, and are aware they cannot drink water or gather in cereal crops, or mow down grass, without destroying animals, they prefer not to kill unnecessarily. There are others who eat fish, flesh, and fowl, to obtain which they regret that animals are made to suffer more or less pain, but object to experiments on living creatures, even if it be established that such experiments cause less pain than the oyster feels which is opened for their wants. There are others who do not reject animal food, and do not oppose those experiments, said by physiologists to be necessary, when it is absolutely certain they do not cause the least pain. There are others who think the interests of human beings demand the occasional infliction of suffering on animals, and object therefore to limit the physiologist to painless experiments, while endeavouring generally to protect animals liable to Vivisection. There is another class who decline to consider

the subject on the ground of pain, and while anxious to prevent wanton and reckless suffering, are willing to leave the nature and number of experiments to the discretion of physiologists. And there are a few only who think animals may be used for food, or for experiment whenever thought necessary for scientific purposes; and, though they would not permit the ordinary acts of cruelty committed by brutal people in the streets, they are prepared to tolerate any amount of pain committed by vivisectors "in the interests of general humanity." It will be manifest that, with such divergence of opinion in the same body, it is the duty of the Executive to adhere strictly to simple principles, to promote which the Society was established fifty years ago, and still exists.

The object of the Royal Society for the Prevention of Cruelty to Animals is obviously to prevent cruelty to animals, and nothing else. The lower fellow-creatures of man are under man's dominion, many species having being reclaimed from a condition in which they had the advantage of freedom, and placed by him in forced obedience to his will and wants. Some he uses for food, others for labour, and nearly all as material in manufactures. The universal practice of society in this respect assumes that man has not only superior rights to these lower fellow-creatures, but that he may ignore some of their rights when he pleases. For instance, he may put them into slavery, and he may eat them. It is true, and must be insisted on, that in this law of civilized life there is no presumption of a right to inflict torture on animals; nevertheless, negatively to some extent they are illtreated even when killed for food by the most merciful methods, and when used for labour by the most humane masters. Fish in sportive enjoyment are abruptly taken from waters congenial to their natures, and deprived of further pleasure. Birds soaring in glorious freedom and happy in song are shot down early in life, and while their capacity for pleasure is unimpaired. Some of these creatures, and many quadrupeds, are indeed brought into existence by man solely that he may kill them for his own food while they are still young and vigorous. Horses, asses, oxen, camels, elephants, and other animals, are deprived of freedom and worked under mechanical conditions to prevent their escape, and without taking into consideration their normal instincts and desires. The reader may judge for himself whether this is a fancied or real infringement of the love of life and freedom, which his lower fellow-creatures are endowed with as well as himself, if he will for a moment imagine the existence of a superior race of beings to man having a propensity to eat him when they desire food, and to put him into the captivity of chains and straps to minister to their pleasure or wants. Doubtless mankind would think it hard and cruel to be deprived of life and of freedom to supply superior beings with supposed necessities. That animals reduced to labour may be made happy is obviously true, which also could be said of human slaves, whose

captivity nevertheless, like that of birds in small cages, has been considered inhumane ; but nobody will contend that human or other creatures capable of happiness are kindly treated by being deprived of life, even though destroyed only for building up a higher race. Thus it will be acknowledged that some of the rights of animals are ignored or abridged by man's assumed rights.

Starting then with this admission, civilized communities have established laws to regulate man's relations with those species of his lower fellow-creatures whom he domesticates and trains for his own wants. In his use of them, abuses may spring up ; hence it has been provided that those animals shall not be cruelly illtreated, abused, or tortured, when human wants bring man in contact with them. These laws are enforced by the Society in Jernyn Street, which is the Parent of all similar institutions in the United Kingdom and the world. It is the duty, therefore, of that Society to interpose when butchers, drovers, carmen, and others cause animals protected by the statute to endure—what ? The answer to this question is very important. The statute does not forbid the employment of animals as beasts of burden and draught, nor does it prohibit the pain of killing by the butcher, nor does it proscribe pain inflicted for reasonable correction, nor does it prevent the veterinary surgeon from performing a surgical operation on an animal for its own good, even though such operation cause much suffering. It provides that man may not "*cruelly*" inflict pain—that is, he may not cause unnecessary pain ; for cruelty is the infliction of *unnecessary* pain.

In order to prevent the abuse of cruelty it has been shown that the legislature has not thought it necessary to forbid the use of animals. It permits a man to take a horse and use it as he pleases, provided he do not cause needless pain to it. And this applies to all relations of man with those animals which are shielded by the statute from cruelty. So that not only cabmen, carmen, coachmen, drovers, butchers, and farmers, must obey the law, but all classes, including those who take animals for the purpose of subjecting them to experiments. The law makes no exemption in favour of persons professing to have high motives ; it simply condemns those who inflict unnecessary pain. The trainer of animals therefore often finds that he may not cruelly illtreat an animal under a false pretence, just as vivisectors proved to have performed unnecessary experiments of a painful kind on domestic animals are condemned by the present statute. The law permits them to use a dog for experiments, but it restrains them from torturing it during or by that experiment ; so that up to the present moment painless experiments on living animals may be carried on by physiologists. Much confusion has been caused by failing to keep this distinction in mind during the discussion on experimental physiology ; for even Vivisection may be, and frequently is, an absolutely painless operation. Discrimination and accuracy are essential to an intelligent and just conclusion. The Society has never failed to distinguish between ex-

periments causing pain and those that are painless, simply because as stated above its functions have reference only to cruelty wheresoever and by whomsoever committed.

It is a reasonable question, put many times during the past few months.—While the minds of English men and women are being distracted by recitals of horribly painful experiments reported in newspapers, what does the Royal Society, whose business it is to prevent cruelty to animals, say as to the accuracy of those statements and the continuance of Vivisection? And it has appeared equally reasonable that information should be published by such Society, to guide the public mind.

The Executive Committee of the Society have necessarily been compelled to observe much reticence lately in speaking or writing on Vivisection. In the month of January, 1875, a deputation presented a memorial to them which contained allegations and prayers (see p. I.). The President and Committee at once promised to give their earnest, careful attention to the investigation of the allegations, and their consideration to the appeal of such urgent memorial. Hence they became charged with a solemn judicial duty, which temporarily sealed their lips—until the proper time should arrive for publishing their report, and for taking those active measures in Parliament which might be expected to result from their deliberations. Before, however, they had concluded the arduous duty imposed on them, including the preparation of a legislative measure, proceedings were commenced in Parliament late in the session without their sanction, to which they could not give their entire support. Ultimately a Royal Commission was appointed to consider the subject of Vivisection, and an intimation was forwarded to the Society that its evidence would be required by such Commission. It will be obvious to all persons conversant with public business that during the period of its judicial inquiry, and the later period when called upon to appear before the Royal Commission as a witness, the Society was bound in honour, as well as decency, not to stand forth publicly as an advocate, or partisan; and that, therefore, with some limitations, which did not in any way forbid the exercise of its private influence and the circulation of information, it has been compelled to keep silence.

No one ought, however, to doubt that the Society will do its utmost to prevent cruelty, whether performed by the ignorant and brutal, or by a callous learned professor. The history of its proceedings is a sufficient guarantee and testimonial of its impartiality; and undoubtedly so in relation to the subject of Vivisection. The past sixteen years have witnessed a series of consistent exertions made by the Executive to prevent painful experimentation on animals. To this end deputations have waited on learned bodies in this country and in France; Congresses have been called; papers have been written; newspaper articles have been published; prizes have been awarded to writers of best essays; innumerable letters have been forwarded to,

and general discussion has been stimulated in, the public journals; the surveillance of detectives has been carried on; and, lastly, the prosecution of offenders has been vigorously pursued. They have not been found wanting either in prudence, decision, determination, energy, or courage. The only prosecution ever instituted against vivisectors under a penal statute, the celebrated Norwich proceedings (which by the way was the primary cause of the present discussion of Vivisection), may be cited as an instance in proof. In opposition to the wishes of a powerful and learned profession, the Committee carried their sense of duty out into active proceedings without faltering, and regardless of the obloquy which medical writers might endeavour to fasten on the Society's reputation.

A brief narrative of further action taken by them on this subject, to which the reader is now referred (see pp. xxiv to xxix), shows further that the Committee have devoted their best energies to this inquiry, exercising the utmost care as well as industry in their deliberations; that they have arrived at a well-defined determination of opinion and policy; and that by declaring those opinions and that policy to the Royal Commission they have nailed their colours to the mast. This volume will set these forth, particularly in the Bill (see p. liii), designed to show the kind of legislation needed to prevent cruel experiments, either by conferring the additional powers suggested on the present statute (12 & 13 Vic. c. 92), or by enacting a separate Act of Parliament. The provisions of this Bill draw a prohibitory line at pain in all experiments to be made. Consistently with all its other operations, the Society's object therein is to prevent cruelty. Discrimination between painful and painless experiments in reference to this question is not a matter of choice, but of necessity arising out of its functions. More than ten years ago, when it invited a competition of Essayists on this subject it stated its question with distinctness. "Is Vivisection necessary or justifiable for the general purposes of science, and if so under what limitations?" In its preface to the Prize Essays, which were published by order of the Committee (Mr. Fleming's and Dr. Markham's), the Society stated, as a reason for its interposition, "that there existed great and reprehensible cruelty in the performance of experiments upon animals, and that important limitations were needed in the practice of vivisectors." The "limitations" alluded to in the above quotations had reference to the infliction of pain. Again, at the Anniversary meeting of the Society in 1863, Dr. Richardson appeared on the platform to vindicate experimental physiology, and after sundry remarks thereon stated that during the entire performance of the experiments, which he contended were necessary, no pain need be inflicted on animals. At once the President (the Earl of Harrowby), with the entire approval of the members of the Society present, publicly stated that the learned gentleman had been fighting a shadow; for the Society did not denounce painless experiments as cruel; but only those which inflicted

pain on animals. In their petitions to Parliament, the Committee have also guarded themselves against an appearance of exaggeration on this subject, by asking that "painful" experiments may be entirely abolished by legislation, without reference to painless experimentation. Later still, the Committee have recorded a similar distinction in their evidence to the Royal Commission (see p. xxix), by urging it to be "essential that anæsthesia shall be insisted on whenever experiments are performed, and continued during the experiments;" and in their Bill (see p. liii), which provides for absolute insensibility to pain during and after an experiment. At an International Congress of Societies for the Prevention of Cruelty to Animals, recently held in London, the subject of Vivisection was discussed, and the same wise distinction was observed between painful and painless experiments. The following resolution was unanimously adopted on that occasion, which it will be seen does not seek entirely to prohibit painful operations, and in that respect falls short of the Society's Bill:—

That painful experiments on living animals, if not already illegal, should be forbidden by law; except when under licence, and under precautions for publicity: and that no experiments on living animals be permitted except under the same precautions—[i.e. not even painless experiments, without precautions for making known the process, the anæsthetic and the animal used, and the result].

In his Essay on Vivisection, Mr. Fleming though opposing experimentation, also made a distinction between painful and painless operations, while admitting the necessity of occasional painful experiments:—

The inevitable conclusion to which an impartial examiner will be brought is, that living dissections are not to be tolerated except upon the most urgent and imperative occasions; *and when every other means has been exhausted it is in the highest degree unjustifiable to sacrifice animals, especially by torturing and causing them pain, for the mere sake of advancing science, and in a minor degree for the purpose of extending our knowledge of disease or sparing human suffering, until every other department of science which can minister to this research has been exhausted thoroughly, and in vain; and then only when there is something like certainty, not mere speculation, that the experiment will confer some significant boon upon the healing art.*

It will be seen then that the Society has acted on this question with consistency. In order to prevent further mischief by dissension at a juncture when disunion is criminal, no reply shall be made here to the inaccuracies of persons who, it must be regretted, have maintained little command over their lips and pens when alluding to a Society which, rightly or wrongly, is employing its best energies against the cruel practice of Vivisection. In its corporate capacity the Society exists only for the prevention of cruelty to animals, and in that corporate capacity it declares war against the cruelty of experimental physiology, and cannot, without abdicating or overstepping its functions, consent either to legalise painful experiments, or to prohibit those which are not only infinitely less painful than the ordinary process of killing, but which are absolutely painless.

The Royal Commission, appointed 22nd of June, 1875, consisted of the following members—Lord Cardwell (chairman), Lord Winmarleigh, Right Hon. W. E. Forster, Sir John B. Karlake, Professor

Huxley, John E. Erichsen, Esq., and Richard Holt Hutton, Esq., whose report, dated January 8th, 1876, was unanimously signed (see p. 215), Mr. Hutton adding a rider in favour of special legislation for dogs and cats. Many comments have been made on the composition of the Commission, into which it would be impertinent to enter. It should be stated, however, that its members are eminent men whose opinions, so far as they are known, represent all those public bodies who have professed to be interested in this subject. Three were at least earnest representatives of animals—Lord Cardwell, Mr. Forster, and Mr. Hutton; the latter being also it may be said a decided representative of anti-vivisectionists.

It was thought by the Society that proceedings before the Royal Commission should be thrown open, that reporters of the Press should be present, that evidence should be taken on oath, and that witnesses should be examined and cross-examined by counsel. These suggestions were found to be unusual or inconvenient for sufficient reasons, no doubt. If counsel had been employed the sittings would have been prolonged, the examinations more exciting, and the evidence perhaps more conflicting. On the other hand, the judges would then have been relieved from the necessity of putting questions on either side, of a nature to lead persons unacquainted with public inquiries to designate certain Commissioners "counsel for Vivisection" and "counsel against Vivisection." The answer to all serious objections appears to be (1) that earnest lovers of animals regularly attended the sittings as judges, (2) that apparently they invited all the witnesses to appear whose names had been given to them on our side of the question, (3) that they called the most eminent medical men and physiologists, (4) that they watched all the proceedings attentively, (5) that the two Commissioners, who are supposed to favour science more than our doctrines of humanity, nevertheless arrived at a decision in accord with theirs. It would have been well, perhaps, if laboratory assistants had been called to prove how animals are treated in laboratories before experiments are performed on them, and afterwards (when not destroyed by the experiments), the number of animals used, where and how they are procured, and whether students practice experiments secretly or otherwise.

Mr. George Fleming, whose name is honourably connected with opposition to vivisectional abuses, was invited to appear before the Royal Commission. In reply, that gentleman referred to his Prize Essay, which had been put in by Mr. Colam, stating it contained all he had to say on the subject. It is singular that Dr. Benjamin Richardson was not called, whose name stands prominent among physiologists especially as he was mentioned by one of the witnesses as performing vivisections at his own house. A statement made by him in 1862 will be found on page 218, and may serve to express his opinions, and explain the evidence alluded to. A further omission was made by the non-appearance of Dr. Cooke, of 30, Gower Street, who ad-

vertises (see Mr. Colam's evidence p. 122) that "Vivisections are gone through carefully every three months" in his classes of students.

The Royal Commission have come to the conclusion that painful experiments are necessary for original research. While this must be deplored, and the Houses of Parliament be urged not to legalise the infliction of cruelty, it must be admitted that such opinion is predominant in the evidence. All the medical witnesses, save two, declare experiments to be indispensable, and all who were asked state that these need seldom be painful to animals.

The Royal Commission countenance Vivisection for teaching purposes when the experiments are performed without pain. No doubt reasons were given to them by professors in favour of this practice, though other teachers thought it unimportant. It would seem hardly wise to familiarise young students with illustrations of mutilated living animals. Such an exhibition is suggestive, to say the least, of an easy way by which to give force to a dilettante evening séance in a private parlour laboratory.

With the exception of the above two points, the Royal Commission have consented to all the provisions of the Society's Bill.

The plan proposed by Mr. Hoggan (see p. 176) is in the following respects objectionable to the Society, and, doubtless, will be also to the reader. It legalises painful experiments, it provides a hall for their performance, it makes them a sensational spectacle to be "overlooked by a gallery into which the public should have unrestrained access," including students who might assist in an experiment, and with a programme of operations to be performed suspended in the gallery. The proposal, as though designed to multiply instead of restricting the number of experiments, gives any person or corporate body power to build a Vivisection Hall, and, after inspection by a government officer, provides that such Hall shall be licensed and opened for experiments. Though well-intended, such an arrangement would not only popularise and increase vivisection, but would demoralise persons who, to gratify morbid tastes, would obtain admission—a result which all opponents of Vivisection deprecate as earnestly as Dr. Haughton does, who says he would shrink with horror from introducing students into laboratories to witness painful experiments, and thus let loose on the world "a set of young devils."

The publication of the Report of the Royal Commission cannot fail to do good. In the first place it will dissipate certain errors which have been fostered by false reasonings on our side of the question. The opinions of eminent men are now declared, which formerly were surmised only. Discussions have taken place, not of the most elaborate, or exhaustive, or even luminous nature; but still light has been let in. Facts have been established which before were doubted or denied. Experiments have been put on their trial, which had heretofore not been made public. It may be assumed that humanitarians have produced their best evidence, and vivisectors

have presented their best defence. Was it a fair contest? Was either party handicapped? A few laymen can scarcely be considered a match for an entire learned profession. If earnestness gives strength it also exposes weakness, and cannot parry the lance of a skilful antagonist. The scientific witnesses, roused to anger by a sense of stigma placed on their order, were also unguarded, and in some instances have incautiously shown their hands. A few of these demand to demonstrate in the presence of students by vivisections, even without anæsthetics; one boasts that he narcotises only for his own convenience and entirely without regard to the feelings of the animals (it is a matter for congratulation that he is not an Englishman), and that the infliction of pain is of no consequence when it does not interfere with the experiment; others contend that competent members of their profession should be placed above all law as regards the torture of animals, and object to interference of any kind, even when covering dogs with turpentine and setting fire to them, or when baking, boiling, freezing, choking, poisoning, starving, blinding, disembowelling others, or dissecting out their nerves, excising their kidneys or portions of their livers, breaking into their craniums and removing their brains, or when pregnant squeezing out their fetuses by gentle (!) pressure. The advocates of unrestrained private experimentation really contend for the perpetration of the above horrors, of which they are cognizant, if not in their own practice, by perusal of medical journals. While the principal vivisectors in this country are more favorable to our principles than could be expected, others evince hostility which can be controlled only by statutory law. This knowledge is a great gain.

The Times, in alliance with *The British Medical Journal*, avers that no abuse has been proved. If so, why do the Commissioners ask for legislation? At present, anyone may take a knife and cut into the sensitive parts of an animal under a plea of scientific investigation. The Commission would prevent this, and permit only licensed persons. If there have been no abuses, why make this restriction? At present operators may omit anæsthetics if they have "no time" to attend to the cries of an animal, or find it convenient not to narcotise. The Commission would compel them, on peril of losing their license, to anæsthetise when possible. Why is this stringent regulation needed if abuses have not been proved? At present experiments are performed in strictest privacy, and no register is kept of the number of animals used or of the torture inflicted. The Commission object to this, and insist on efficient inspection and full returns. Why should private laboratories be invaded if no abuses have been detected? At present a student or physiologist or any private person, may practice Vivisection in his private house with impunity. The Commission would give the Police power to break into such house by warrant, and search for alleged offences. Why this severe interference with personal liberty and rights if no abuses

have been discovered? The truth is, the Commissioners have judiciously avoided alluding to instances of cruelty proved, leaving the public to read the evidence for themselves, and to infer from it, and from their application for legislation, that abuses have been discovered requiring even the check of Parliament, although cruelty may have been committed less frequently in England than is generally believed. Of course, the legal restraints suggested have reference also to the future, as well as to the past and present; because this investigation has brought to light the fact that experiments are likely to increase in number, and that the dust thrown into the eyes of good people by certain medical journalists, in prophesying an approaching cessation of experimentation, is only a cry of "peace, peace."

A few mistakes have been made by humanitarians, arising, it may now be said, partly from the secrecy of experimentation and the consequent difficulty of acquiring correct information. It surely is illogical to contend that such circumstance nullifies the ample evidence of actual cruelty cited and established by several witnesses. If the accumulated evidence of suffering presented in the name of science at this inquiry, testimony which cannot be gainsaid by negative statements, fail to prove abuses, then a new meaning must be found for the word "abuse."

Furthermore, has not even the *Handbook for the Physiological Laboratory* been apologised for or condemned by the witnesses who wrote it, the majority of other witnesses, and the Royal Commission; and have not certain experiments, referred to even by vivisectionists with reprobation as causing unnecessary pain, been generally condemned by this high court of humanity to brutes? And have not the two medical Commissioners been induced to agree in recommendations for legislation? This circumstance alone is a condemnation of unrestrained Vivisection. Moreover, the Blue Book is now being read by thousands of people. Its facts have been placed on record, and cannot be lost to our cause, or be got rid of by the specious reasonings of medical journalists. The publication of the Report of the Royal Commission cannot, therefore, fail to do good.

The friends of exaggeration should, however, take warning by the evils which a want of care inevitably brings. Liberal allowance ought to be made, and will be made, by lovers of animals for the natural excitement of persons, whose thoughts are haunted day and night by the details of torture which undoubtedly has been committed by physiologists, as shown by the evidence published in this volume. Others, whose temperament and want of business habits do not dispose them to weigh well the words they are prone to utter, have probably been seduced into hyperbole. The enemies of tenderness to animals are vigilant to seize upon weakness of this kind, and flaunt it before the world against our principles, instead of showing any grace of forbearance. In this way trump cards have been played into the hands of physiologists by writers, who e.g. have quoted the

atrocities at Alfort as proofs against Vivisection performed in their own country. Those shocking operations are denounced by physiologists quite as severely as by humanitarians ; they are defended by only a few French veterinarians, not for the study of physiology, but for the acquisition of manual dexterity ; and are not performed in this country and never have been. Hence, the folly of mixing them up with very different experiments which prevail in England, or with others countenanced by physiologists abroad. Again, a random accusation to the effect that Vivisection is Vivisection, and its practice attended with as much pain whenever and wherever and by whomsoever conducted, all difference in the methods used by operators notwithstanding, and that English physiologists are equally cruel in their operations on animals as foreign experimenters, has been made admittedly without proof, and certainly against the common experience of persons acquainted with the facts. Again, descriptions of experiments, of implements of experiments, of conduct and conversations of experimenters in laboratories, have been published in papers in reference to the present agitation in England, conveying impressions that such matters relate to English vivisections. Exposures of this nature are valuable, provided they do not mislead. It is not always convenient to identify ; but in a discussion on English practices, it is incumbent on a writer to exonerate English physiologists, when narrating particular and definite circumstances in which he knows they are not blamable, while certain foreign physiologists are, instead of leaving them under the stigma of his terrible recital. Such omissions may serve a temporary purpose, but when discovered, like actual mis-statements, they lead to unfavourable conclusions. Again, it has been published, without corroboration of any kind, that curare intensifies pain, and persons who use it have been represented as fiends gloating over increased agony. The truth appears to be that curare does not intensify pain, that it is not a safe anæsthetic, that it is very doubtful whether it lessens pain at all, but it prevents muscular movement. Again, experimenters on animals have been accused of indulging in wanton cruelty (not in simple cruelty, but in *wanton* cruelty), by cutting into the nerves of animals to satisfy an idle, morbid, or sportive curiosity only, or to gratify a devilish love of torture, and without any object of physiological research. Obviously this should not be said without evidence of facts ; for as cruel motive is difficult to establish, and as the accusers are considered technically disqualified to show an absence of scientific motive, such terrible accusations react against our cause when proofs are asked for and cannot be given. It is enough for our purpose to reprobate cruelty when it is clearly disclosed, without entering into the motives of the perpetrator. What is the result of these and similar errors in the progress of our work ? Before the appointment and during the sittings of the Royal Commission, a kind of panic had been raised. Journalists, giving credence to statements not always

well based, wrote indignantly in their newspapers, evidently showing a favourable animus to our objects. Upon the appearance of the Report of the Royal Commission these writers yielded to a reaction of disappointment. Their expectations of a strong case had been raised partly on sensational, irrelevant, and imperfect data—they felt aggrieved, and blaming everyone except themselves, and without examination of the actual evidence, which God forbid should ever become pregnant with worse cruelty, they committed themselves more or less to the opposite party—averring that “no abuse had been proved” (see *The Times*), and that “the shouting had been all on one side” (see *The Daily News*). Even on the minds of the Commissioners the overstatements alluded to seemed to have a pernicious effect during the inquiry. These are lessons that must be heeded by all who are sincerely anxious to protect animals, instead of exercising their powers of invective; and particularly at a moment when legislation may be introduced by the Government, and false tactics may retard the progress of a combined opposition against the atrocities of Vivisection.

The Society’s Bill absolutely prohibits Vivisection for illustration in teaching (see section 7), and for attaining manual dexterity (see section 8). It absolutely prohibits Vivisection in the absence of complete insensibility (see sections 9 and 10). It provides means by which officers may enter private houses and catch offenders redhanded (see section 12). It compels persons who perform even painless operations to use a place for such purposes where any cruel act will be discovered (see section 2). It provides that a register shall be kept of all such painless experiments to satisfy the public that the Act has been obeyed, such register to be open to inspection, (see section 14).

It may be submitted that if any law can have effect on offenders, the Society’s Bill would be found effectual. All laws are broken in secret, even those which say “Thou shalt not kill,” “Thou shalt not steal;” though they restrain many men, a few endeavour to evade their consequences by cunning concealment. When caught, punishment ensues. The same consequence would follow the enactment of the Society’s Bill. It would restrain many, but some offenders probably would operate in secret, and without anæsthetics. These would surely be punished when overtaken. If a law existed for entirely abolishing experiments, the same results would take place—concealment of Vivisection, and punishment upon detection. Objection, therefore, to this Bill, on the ground that it would not always be obeyed, is practically an objection against all laws.

It might with equal force have been said to Martin, “What good will your Bill be; men will obey it when under the eye of a constable, perhaps, but they will thrash their horses when no one is looking on in the stable?” Was not the answer obvious? To prevent any cruelty will be a gain, even though the frightened ruffianly perpetrator be not always restrained; but this law will lay down a principle which will

Reference to this page from third line of p. xxiv. should have been to p. 1.

restrain hundreds of men who are not radically cruel, but only thoughtless; and it will educate the public mind so soon as its provisions are carried out by magistrates. The experience of the Society justifies such conclusion. What has altered the feeling of the general public on the subject of cockfighting, but the enactment and application of the statute? Mr. William Howitt as a writer is entitled to an opinion, but his experience, as regards the application of a law on this subject, is of little worth in comparison with that of a Society whose whole history is more or less associated with the detection of offenders, and the prevention of cruelty. It is astounding that he should have ventured with so much vehemence in a field where his own knowledge cannot guide, and his judgment must be governed by abstract theories, and not by practical experience. The mere passing of the Act against cockfighting has considerably deterred offenders. Admiral Rous, in a characteristic letter lately published by *The Times*, admitted that but for the law he would have continued to fight cocks. When that law was enacted, many of the prominent men of England were "cockers," and the sport was general and openly indulged in. The change caused by the law, and its application by the Society, are not only salutary to public morals but wonderful in completeness. Only roughs dare now be seen at cockfights. Gentlemen, so called, protect themselves by a cordon of scouts when about to enjoy a "main;" and, if surprised during the performance, they conceal themselves in ignominious places, offer any amount of money as bribes to evade justice, take to their heels, or like Richard offer a kingdom for a horse. At the time when it was proposed to add the sections against cockfighting to Martin's Act, certain Mr. Howitt's prognosticated with sneers that the practice would nevertheless prevail in dens cleverly contrived, shrouded in profound concealment, sentinelled by adroit scouts, under the outspread wings of an equivocal law. The result shows that there were bad prophets in those days. For even without a section giving power to break into the dens alluded to (which the Bill under consideration provides), the practice has been put down as effectually as any vice can be suppressed. Now and then it crops up in mining districts, where a bloated prosperity enables colliers to indulge in the pastimes left off by their betters. The Society's officers have again and again entered those dens and dragged out the offenders, "gentlemen" and roughs together, who have been punished by magistrates and by public opinion. The disgrace which a penal law puts on a wrongful act, not only restrains offenders, but levels up the mind of the entire community; and in this respect it performs the highest possible moral service.

Let it be assumed that a Bill abolishing all experiments, painless as well as painful, were already passed. How would its provisions be carried out to suppress private operations, except as provided by the Bill of the Society? If the latter be inoperative to overtake offenders, the former would also fail, for the same conditions of secrecy would

meet both Bills. But the Society's Bill, while comprising the machinery requisite for the prevention of illicit Vivisection, contains other provisions of essential value. It forbids all secret experimentation, and all painful experimentation; and it permits experiments only in a place where an inspector shall be stationed to prevent pain—an inspector who shall be trusted by the public, of course. It practically says therefore:—"If you are determined to experiment you shall do it here in our presence, and we will take care that the animal shall not suffer an atom of pain; and if you dare to do it elsewhere we will break into your house and bring you before a penal tribunal." Can the most advanced "Abolitionist" do more? If in the place provided anæsthetics be not applied, it will be the fault of inspection. It is idle to contend that anæsthetics do not prevent pain. Into what absurdities will infatuation lead people! If we go into our hospitals, we may see legs and arms amputated from the trunks of living men and women under anæsthetics without pain. Under anæsthesia, a horse belonging to Sir William Erle stood without halter and bridle in the stable yard while a large tumour was cut out of its body. And yet people profess not to believe in the efficacy of anæsthesia. The Queen expresses a hope that anæsthetics, which have proved to be a great blessing to man, may be used in experiments on animals, and forty-six out of the forty-seven medical witnesses before the Royal Commission aver that anæsthetics when properly applied ensure absolute insensibility. The Society reasonably lays hold of these facts of science and this important evidence, and asks for their application to legislation.

If anæsthetics can be applied in four hundred and ninety-nine out of five hundred experiments, as Dr. Pavy alleges they may be, then their use should be compulsory. If they can be enforced under inspection, and obviously they can be, then inspection should be compulsory and efficient. If by such means the animal can be preserved from all pain, and the evidence on the practicability of perfect anæsthesia is conclusive (see *Tabular Digest* after the last page), then the blessings of this discovery ought to be extended to animals, as the Queen observes in her letter to the Society.

The arguments that apply with much force against enactments, which legalise cruelty in order to restrict Vivisection, cannot be used against this Bill. It does not in any way legalise, but absolutely forbids cruelty. If it be urged that the Bill is not required, then it may be asked, how is Vivisection to be stopped? The present statute against cruelty (12 and 13 Vic., ch. 92), even if it apply to acts of cruelty performed by vivisectors (and it must be held to do so when the act can be proved to be cruel, unnecessary, and perpetrated on a domestic animal) extends no shield over thousands of animals tortured in the process of physiological inquiry, and does not provide for the issue of a search warrant against offenders. Besides, it leaves operators in the exercise of a right they have at present to experiment

in secret, and demands only that no animal shall be tortured. If they may operate in secret, without any power of forcible entry by the police, how can anyone be assured that animals are not tortured, the Act notwithstanding ; and how can such cruelty be prevented under the present Act ? But if the legislature enact that, as in other instances where inspection is necessary, a place shall be licensed at which, and at no other place, even painless experiments shall be made, then a way will be opened for the prevention of cruelty during such experiments, and for punishing persons who secretly subject animals to Vivisection. Two examples may be given to show the force of this argument.

The present statute against cruelty enables officers to enter certain licensed places where horses are killed, and thus the merciful treatment of infirm horses is ensured by being under the eye of the Society and of the police. Imagine that no such places exist, and that horses may be destroyed anywhere ; and the advantage of a licensed place becomes apparent. Again, the 1500 slaughter-houses of London are private property, and officers can enter them to inspect the conduct of butchers only on sufferance. If these private houses were abolished, and abattoirs established at twelve different points of the metropolis, then the slaughtering of cattle might be under surveillance for the prevention of cruelty. At present, as in Vivisection, that is impossible.

Hence evidence cannot be got against present offenders, even if the Act could be made applicable to physiological experiments. If means had existed for detecting cruel experiments, the Society would have been quick to prosecute. All the evidence it has recently submitted to the Royal Commission it has culled from books, which would be utterly useless in prosecutions without the sworn testimony of persons. Again, another difficulty must be considered at this point. Comparing the present statute with the Society's Bill, it will be seen that if the Bill be objectionable because it permits painless experiments, so is the present statute equally objectionable ; for that does not forbid painless operations on animals, and a vivisector could not be punished under its provisions who had dissected out the great sympathetic nerve of a dog before the eye of the Secretary of the Society, provided he performed such Vivisection under an anæsthetic, and destroyed the animal while insensibility remained. So that at the present moment the law legalises experiments of a painless nature, and the Bill of the Society entirely accords with its provisions in this respect, but adopts a plan for getting information which hitherto has been unattainable. Whether the powers of the Bill be enacted as a separate Act or in enlargement of 12 & 13 Victoria, ch. 92, is immaterial. The really important consideration is—Shall this state of things last, and cruelty go undetected in compliance with a sentiment, misplaced when applied to the provisions of this Bill, seeing that it does not legalise any kind of cruelty, but on the contrary prohibits it with a stern hand.

The objections against any enactment which shall tolerate painful experiments are formidable. Such legislation would be retrogressive, and could have no support from the Society. It would repeal the present statute of humanity to animals in part. This is obvious upon a little reflection, and may be proved by reference to the Bill, introduced into the House of Commons during last session by Dr. Playfair, in the enacting clauses of which the following words appear—"notwithstanding the Prevention of Cruelty to Animals Act" etc. In the United States of America the laws which protect animals are in every other respect superior to our own, but they permit Vivisection; and the Societies there have tried in vain to obliterate such blots from their statute books. The *Daily News* justly observes there is damage in granting licenses to perform "painful experiment. The legalisation can be done at a word; the restriction is infinitely more difficult," having once tolerated the infliction of suffering; and these thoughts are expressed in a still more forcible manner by the Baroness Burdett-Coutts, who says, "Let us pause before an Act, or any portion of an Act, is infringed which keeps cruelty in check—which renders illegal the torture and abuse of animals, and which should be made the means of inculcating consideration, tenderness, and justice."

The reader will judge from the contents of this volume whether the Society succeed in proving (1.) That vivisectional experiments on animals are frequently used by medical men and physiologists in this country. (2.) That in many instances no anæsthetic agent is employed throughout the experiments, when much pain is necessarily caused. (3.) That the alleged object of such operations is scientific research and demonstration. (4.) That the same experiment is repeated many times on different animals. (5.) That an effort is being made to instruct students, by means of handbooks, in the practice of vivisections. (6.) That professors exhibit living animals, previously narcotised and operated on, to students in classes for teaching purposes. (7.) That at least in a few instances students have performed experiments secretly. (8.) That, although English physiologists have hitherto been more merciful to animals than foreign experimenters, there are alarming symptoms of continental methods and habits being introduced into England. (9.) That no experiments should be performed which cause pain. (10.) That medical students should not be permitted to experiment or to see experiments. (11.) That even painless experiments should not be tolerated except under regulations to prevent excesses. (12.) That in many experiments, and notably some of those in which animals cannot be relieved of pain, the results do not appear reliable.

The leading feature of the Society's Bill is the prohibition of all experiments which cause pain. It may be said by vivisectionists that this provision is irrational, and would abolish many useful inquiries. It is nevertheless humane and just to the claims of animals; and that is its recommendation.

MINUTES OF MR. COLAM'S EVIDENCE BEFORE THE
ROYAL COMMISSION ON VIVISECTION.

1515. (CHAIRMAN.) You are the Secretary of the Royal Society for the Prevention of Cruelty to Animals?—I am.

1516. How long have you been so?—Fifteen years.

1517. During that time I presume it has been your province to carry into execution the wishes of the society, as far as you could, in reference to the subject which we are appointed to consider?—Yes.

1518. Has that given you the means of knowing, with any considerable degree of accuracy, what is the actual practice in this country on the subject. Do you know, that is to say, what things are done to animals habitually in this country?—Personally, I know a little; and from reading, I think I know a little more.

1519. And can you inform us what is the actual state of things now in existence in this country?—Vivisection is practised of course, and very often with very much pain to the animals; that is undoubted.

1520. Have the society arrived at so much of a conclusion on the subject as to be enabled to put their wishes into the form of a Bill?—They have.

1521. Have you got that Bill with you?—I have.

1522. Are you prepared to give it to the Commissioners?—Yes. (*Handing it in.*—See pp. xv. and xvi.)

1523. Will you tell us, if you please, in general terms, what the provisions of that Bill are?—The main feature is that it prohibits all painful experiments.

1524. By "painful experiments" will you tell us what exactly you mean; is an experiment in its nature painful within the prohibition of your Bill when performed under complete anæsthesia?—The experiment is without pain then.

1525. Then it would not fall within the provisions of your Bill?—Certainly not.

1526. Will you tell us in general terms what the Bill provides?—With regard to that question of inflicting pain, the ninth section provides: "That no person shall perform, or cause to be performed, or take part in performing, any vivisection upon any animal, without

having first of all subjected such animal to the influence of an anæsthetic, so as to render it wholly insensible to pain;" and then the next section provides: "That no person who shall perform, or cause to be performed, or take part in performing, any vivisection upon an animal so subjected as aforesaid, shall omit to destroy such animal, before the effect of the anæsthetic ceases."

1527. Then you seek entirely to prohibit the infliction of any pain upon any animal for the purposes of scientific investigation or discovery?—Yes, that is so.

1528. Now suppose the case to be this: that an experiment is performed under an anæsthetic, and the actual operation is not therefore painful to the animal, but suppose the recovery from that operation to involve some pain; that would be an operation prohibited by your Bill, or your Bill would require that the animal should be destroyed?—Undoubtedly.

1529. So that if an experiment were tried for the purpose, for instance, of improving the mode of tying arteries, and the principal pain were removed by an anæsthetic, it would be contrary to your Bill if the animal were allowed to recover afterwards, and there were some pain in the process of recovery?—Yes, I think it would be, because it provides that the animal must be rendered "wholly insensible to pain," and that the animal must be destroyed while anæsthesia remains. But I would like to add that very frequently the pain is quite as severe after the ligature has been made as it is at the time of the operation.

1530. Now as regards the practice, so far as you are aware of it at the present time, are experiments performed in which no anæsthetic is used?—Undoubtedly.

1531. And experiments in which an anæsthetic might be employed without prejudice to the supposed object of the experiment?—Upon the opinion of others I should say so; but as a layman I am scarcely able to answer that question.

1532. But that is the conclusion at which, as the secretary of the society, you have arrived?—Quite so.

1533. Will you give us some instances which, in your opinion, best illustrate what is actually done?—I have here a mass of evidence, consisting of 370 pages which has been culled from various books reporting experiments, in which there is evidence of pain. I have also other quotations in which there is evidence of prolonged pain. With regard to the question put to me, of course it would be impossible for me to go through this large number of cases. I will just name one or two. It does seem to me that there was a great deal of pain caused by the suffocation of dogs, when a number of animals were drowned and half drowned, and again restored to life, and then drowned again by a committee of the Royal Medical and Chirurgical Society, in consequence of an application by the Royal Humane Society to them, to consider some better means for the resuscitation of persons

apparently drowned. The experiments which are recorded here will show you that the animals were, several of them, two or three times put into water half drowned, and kept in water three, four, or five minutes, and then brought again to life, and thus put to horrible agony. It might be said that the object was very good, but the answer to that is that the cruelty was really fruitless, as the report admits that no conclusion could be drawn from the experiments. The report shows that experiments on dead human bodies were useful. Practically, Dr. Sylvester's method was recommended by the committee appointed by the Medical and Chirurgical Society, which method was in use before the inquiry took place, and is still in use. I wish to add that the Royal Humane Society is not at all responsible for these experiments.

1534. I understand you that you have there 370 pages of evidence, from the length of which you wish us to take them as read, without putting you to the task of reading them through. Did I rightly understand you to say so?—Yes, I put them in for your perusal.

1535. Have you got the references and the names specified?—In every case.

1536. Will you be so good then as to put in the references to those instances, and the authority upon which in each case your statement rests?—I will.

1537. (SIR JOHN KARSLAKE.) I understand that these are extracts written out by yourself from different books?—Yes, or by my orders; altogether about 800 pages of foolscap.

1538. (CHAIRMAN.) Will you at the present moment make a selection of a few instances which you think will best inform the Commission of the points which you desire to bring before them?—I am scarcely prepared to analyse, because I thought that I should just simply have to put my evidence in for your perusal; and, therefore, the manuscript consists of divisions in this form:—Evidence of painful experiments, evidence of pain prolonged, evidence of design to teach students vivisection in laboratories, and evidence of vivisection performed by students themselves, (which I have only found one instance of in the whole of my inquiry), and opinions in favour of restriction, and against vivisection.

1539. You said, I think, that some of the experiments you have collected were instances of protracted agony?—Yes.

1540. So far as you know had any attempts been made to use anæsthetics in these cases?—I believe not. In some cases it may be that it would defeat the object of the experiment if an anæsthetic were used.

1541. But as a matter of fact where there has been an experiment involving what you have called protracted agony, is there any reason to suppose that it has either been possible, or that at any rate it has been the fact that agony has been counteracted by chloroform or other anæsthetic?—No.

1542. In that case it would defeat the object of the experiment to use an anæsthetic?—Yes; generally I think.

1543. In the cases in which it would have been possible so far as time or the object was concerned, do you consider that anæsthetics have been used always?—I believe, that generally the English physiologists have used anæsthetics where they think they can do so with safety to the experiment.

1544. Then may the Commission take your belief to be that there is a desire on the part of the scientific men in this country so far to get rid of the infliction of pain as is compatible with the scientific object which they have in view?—I should say so generally, but in some cases there appeared to be some heedlessness with regard to the suffering of the animal; for instance, in some of Brown-Séquard's experiments, where animals were kept for weeks in suffering.

1545. May I take it to be your view that the general tendency of the English scientific world is not at variance with humanity?—I believe it is very different indeed from the practice of foreign physiologists.

1546. So that you would treat cases of wilful cruelty, if they exist at all in this country, as exceptional cases rather than as fairly chargeable upon any want of proper sentiment on the part of the profession?—Undoubtedly with regard to wanton cruelty. I do not know that I know of a single case of wanton cruelty, by which I mean suffering caused without any object except to gratify a cruel mind.

1547. Then you give the scientific men of this country credit for using anæsthetics, and dealing tenderly with animals so far as is compatible with the objects which they have in view?—Yes; I think so, speaking generally. As regards tenderness, I have no evidence to prove they are tender to animals.

1548. That the cases where that is not so are exceptional cases and not cases fairly chargeable to the profession generally?—I think so.

1549. But you think that experiments are performed which are in their nature beyond any legitimate province of science, and that the pain which they inflict, is pain which is not justifiable to inflict, even for the scientific object which they have in view?—That is the opinion of our society.

1550. Now with regard to students, have you any evidence that you can lay before us as to what is done in respect of students?—With regard to London I have inquired at every school, and I have not found a single place where experiments are actually performed before students. The animal is operated on in the laboratory and brought out thence in a narcotised condition, and then it is examined by the students.

1551. Do I rightly understand you to draw a broad distinction between an experiment performed for the purpose of establishing some

new scientific truth on the one hand, and on the other hand the repetition of such experiment, after the truth in question has been established, for the purpose of illustrating it to students?—Undoubtedly the society draws a very marked line there. A difficult question, of course, does arise as to when the truth has been fully established, but illustration to students is altogether objected to.

1552. But supposing that the object is to establish some new scientific truth, and that anæsthetics are as much employed as the nature of the case admits of, I still understand you to say that the society object unless the anæsthetics can be so far employed as to take away pain altogether?—Yes, the society is a society for the prevention of cruelty, and would not step out of its direction to legalise anything contrary to its nature ; it would be a misnomer for the society as a corporation to legalise cruelty for any purpose. What individuals in the society might be willing to do would be a different matter.

1553. Do I rightly understand you that there are individuals in the society who maintain a different view, but that the society in its corporate capacity speaks through the four corners of this Bill which you have put in?—Undoubtedly.

1554. Does that include this, that some members of the society would probably go beyond this Bill?—Undoubtedly there are some who would prohibit all experiments, and others who desire that no Bill should be framed, believing the present statutes sufficient to suppress cruel experiments.

1555. And there are others who you think might not go so far as this Bill?—Yes, undoubtedly.

1556. But in its corporate capacity the society speaks through this Bill?—Yes

1557. This Bill was framed after the society had had before it the two Bills that were introduced into the two Houses of Parliament last year?—It was ; but we had previously drafted two other Bills.

1558. And of the two it is framed more upon that introduced into the House of Commons by Dr. Lyon Playfair, than upon the other?—I think so ; but certainly it takes some of the provisions of the other Bill.

1559. Now would you have the kindness to state to the Commission what are, in your opinion, the principal differences between the proposals which were made last year to the two Houses of Parliament, and the Bill which on the part of the society you are authorised to submit to us?—The first thing that I note is, that Dr. Lyon Playfair's Bill provided that it should come into operation immediately, and ours provides on the 1st of January. That is perhaps an immaterial matter, but this Bill provides that there shall be no pain inflicted ; that is the leading feature of it. Then, too, it asks for power to go to a justice of the peace and get a search warrant if there is reason to think that the law is being infringed.

1560. That provision perhaps is due to some experience that the

society may have of the way in which their attempts at prosecution have heretofore been defeated?—Undoubtedly; and especially with regard to this question; for now it is next to impossible for the society to know what is going on actually. Then there is another difference which I should like to point out, which is, that there is a more ample provision in this Bill for the registering of experiments, especially with reference to the results of the experiments; the object of the society being two-fold:—first, that it might have an opportunity of testing whether the experiments were doing any good, and secondly, that it might by the publication of the results prevent the repetition of experiments; because if those results were published it would undoubtedly prevent much experimentation.

1561. Your main difference from the Bill of Dr. Playfair is that you omit altogether the provision which he made for the infliction of pain in a certain class of experiments?—Just so.

1562. And you comprehend all painful experiments in the prohibition of your Bill?—Yes.

1563. Then, like Lord Henniker, you adopt the proposal for a license in regard to places?—Yes.

1564. And you extend that to persons?—Yes.

1565. So that if your view were to be adopted by the Legislature no painful experiment could be performed at all?—None.

1566. And no experiment whether painful or under complete anæsthesia, except by licensed persons in licensed places?—None.

1567. Is there anything that you desire to bring before the Commission which has not been included in the questions already put to you?—I do not know that there is, excepting that the Committee are very anxious first of all that you should understand that the Royal Society for the Prevention of Cruelty to Animals are not the society advertised for the total abolition of experiments, and that they are not responsible for the rash literature which has been printed on this subject; and secondly, I was instructed when I came here to read to you what the society have done on this subject, and what the Committee, upon the presentation to them of a memorial against vivisection, did in reference to the examination of the allegations of the memorial, and deliberations upon its prayer.

1568. That is a document prepared under the sanction of the society, and you are speaking their language when you read it to us?—Yes. “To the Honourable Royal Commission on Vivisection. My Lords and Gentlemen,—In response to your invitation, the Royal Society for the Prevention of Cruelty to Animals, have desired me to appear here for the purpose of placing before you the evidence they possess in relation to the practice of vivisection in this country. Instead of a layman, they would have preferred that their representative should be a medical man or a physiologist, whose experience and special education would prove to be of more service to you than my information can possibly be. There are, however, good reasons to

prevent the attendance on their behalf of an expert, some of which may be found in my better acquaintance with their views, and with the proceedings of the society in reference to this practice. They have desired me to place myself entirely at your service. To this end I have already suggested the invitation of several witnesses who may be expected to place information before you. [See p. xlviii.] It will be convenient, perhaps, for me to state (1) what course the society have hitherto taken against vivisection, and what means they have employed to get evidence; and (2) the nature of the information they have obtained and propose to place in your hands. The practices which they opposed many years ago at Alfort and Lyons Veterinary Schools with much success, scarcely come within the meaning of the term 'vivisection,' as they were designed to teach dexterity of manipulation, and were, therefore, more of the nature of operative surgery. As similar practices do not prevail in this country, and, on the contrary, are reprobated by physiologists on this side of the Channel, it will not be necessary to allude further to them. Thirteen years ago I was instructed to make an inquiry into the practice of vivisection in Great Britain, and addressed letters to the schools of medicine, asking for information. The committee of the society were much gratified to find that at such a time there was every willingness on the part of professors to supply the data asked for, and particularly that there appeared to them to be comparatively few operations being made at such institutions. Nevertheless, it was considered desirable to use every possible moral means to check the spread of vivisection. The Committee, with this view, offered prizes for the best essays against that practice, placing two questions before the essayists, as follows:—1. Is vivisection necessary or justifiable when performed (as at certain veterinary schools) for the purpose of giving dexterity to the operator? 2. Is it necessary or justifiable for the general purposes of science; and, if so, under what limitation? Assisted by eminent judges the awards were made, and the Essays by Mr. Fleming and Dr. Markham were printed, a copy of which I now lay before you.* Other and many means were used to the same end. Last year the committee felt bound to institute a prosecution against Dr. Magnan and others at Norwich, for alleged cruelty to dogs during an experiment at the meeting of the British Medical Association. The evidence in that cause, with a report of its result, they put before you on this occasion.* Last year also an International Congress of societies assembled in London to discuss common objects tending to promote kindness to animals, at which resolutions were unanimously passed by the delegates, condemning the growing practice of experimental physiology, so far as it tends to animal suffering, and pledging themselves to no vivisections in the various nations of Europe and America. Owing to the alleged increase of experiments, an influential deputation appeared before the committee last January, and presented

* May be had at 105, Jermyn Street, S.W.

Memorial to the society, praying for its active interference to repress vivisections. A copy of that Memorial is now handed to you, with the names of the principal persons who signed it. [*See* p. xii.] A committee of the society was at once formed, charged with the duty of inquiring into the allegations of the Memorial, and deliberating on its prayer. That committee held frequent and long sittings, and delivered its report to the general committee in due course, the following being the substance thereof:—We have held several meetings, and have taken much pains to obtain evidence to verify the allegations made in the Memorial as regards the extent to which the practice of vivisection prevails in the United Kingdom—where, by whom, and for what objects, experiments are performed; and whether or not animals experimented on are previously made insensible to pain, and kept during experiments, and until destroyed, in a condition of anæsthesia. At every stage of our inquiry we have experienced great difficulty in procuring such evidence. Avenues leading to private laboratories are closed to this society, and private operators, whose conduct is under investigation and public stigma, have not been found willing to volunteer information. Moreover, the gravity and complexity of the subject call for more than ordinary care and exactness in the collection of reliable data. In the first instance we addressed a circular to the several hospitals, infirmaries, and medical schools of the metropolis, to which places the Memorial appeared to refer, a copy of which I hand in to you. The managers were respectively invited to supply particulars relating to operations on living animals in those institutions, and to permit the Secretary to be present on occasions when experiments were being conducted. The following return will shew the results obtained by such measure. No replies have been received from St. Thomas's Hospital, West London Hospital, Infirmary for Consumption and Diseases of the Chest, North London Consumptive Hospital, City Orthopædic Hospital, Harrison Spinal Institution, National Orthopædic Hospital, Royal Orthopædic Hospital, Central London Throat and Ear Hospital, Ear Infirmary, Central London Ophthalmic Hospital, Ophthalmic Hospital, St. Peter's Hospital for Stone and Urinary Diseases, Epidemiological Society. Acknowledgements only have been received from the Medical Society of London, and King's College Hospital. Evasive answers were received from University College Hospital, St. Mark's Hospital, City Road, and Great Northern Hospital. No experiments are performed at East London Hospital, Royal College of Surgeons, Hospital for Diseases of the Nervous System, Western Ophthalmic Hospital, British Hospital for Diseases of the Skin, London Fever Hospital, Royal College of Physicians, Hospital for Hip Diseases in Childhood, Royal Hospital for Diseases of the Chest, National Hospital for the Paralysed and Epileptic, King's College Hospital, London Hospital, Hospital for Consumption and Diseases of the Chest, Royal Dispensary for Diseases of the Ear, City of London Hospital

for Diseases of the Chest, Metropolitan Free Hospital, Obstetrical Society of London, German Hospital, Poplar Hospital for Accidents, Female Lock Hospital and Asylum, Male Ditto, Royal South London Ophthalmic Hospital, St. Mary's Hospital, St. Saviour's Hospital, Royal Free Hospital, Cancer Hospital, Royal London Ophthalmic Hospital, St. Mark's Hospital, London Infirmary for Diseases of the Legs, and Great Northern Hospital. Vivisections are performed at the Westminster Hospital (Broad Sanctuary), Guy's Hospital, and the Pathological Society. Admission has been refused by the Middlesex Hospital (from Medical Committee, not Board of Management), Charing Cross Hospital, Pathological Society of London, Chemical Society of London, and the Royal Medical and Chirurgical Society. The following have consented to admit : Westminster Hospital (qualified persons only), St. Bartholomew's Hospital, Guy's Hospital, the Brown Institution and St. George's Hospital. Mr. Fleming and the Secretary accepted the invitation to attend at Guy's Hospital, and have reported thereon as follows:—

In compliance with an invitation we visited the above Hospital to-day, for the purpose of ascertaining the nature of the experiments performed on animals at the Hospital. During our conversation Dr. Steele said :—‘I can give you positive assurance that students do not perform any experiments on animals here, for Drs. Pavy and Pye-Smith are the only operators on animals. I do not believe there is any cruelty to animals performed in any experiment as anæsthetics are invariably used.’ He handed to us the annexed Hospital Report, and upon our calling his attention to a passage marked at page 48 [that passage seemed to imply that vivisection was performed there before students ; but it was explained to be ‘rather ungrammatical language’], he said it was an unfortunate sentence and that the secretary of the medical department should have qualified it by adding ‘all animals being narcotised.’ He said he was glad to see us for the purpose of correcting wrong impressions and would admit us to the Hospital at any time. He then conducted us to Dr. Pavy's private laboratory, where we saw three living dogs (a long-haired terrier, a pug, and a mongrel) which we were informed were shortly to be the subjects of experimentation. We also saw an operator's table, and several appliances for experiments. Dr. Pavy said during our conversation with him, ‘I am glad you have come to my sanctum to judge for yourselves whether we are cruel or not. There is much misrepresentation abroad on this subject, which no doubt arises out of an unfortunate expression coined by the College of Surgeons some time ago when they desired that students should be taught what they called practical physiology. Students are not, and never have been, permitted to perform experiments at this school, neither do they ever see experiments performed on animals which have not previously been rendered insensible to pain. I will tell you all I do before the students, and privately in this laboratory. Every

animal dissected before students is either narcotised or pithed [an operation on the medulla oblongata which destroys life], except in the case of frogs, which I decapitate in the presence of the students, and before I commence to operate on them, the chloroform is administered, and the pithing is performed in this laboratory before the animal is taken down into the lecture room for demonstration, and the animal is killed before consciousness returns. I would not dare to perform an experiment before the students upon an animal not previously narcotised, any more than I would dare to commit an act of cruelty before your eyes in Jermyn Street. In all my lectures I have always felt that I have not done a single thing, nor said a word I should be afraid of your seeing or hearing; and, indeed, I have felt it my duty to act as though you were present in the room. Could I have been guilty of giving pain to a dog or cat during my lectures, I am sure the students would have been the first to protest against it. I do not use more than eighteen animals during my lectures in a term. He then showed to us his appliances for administering chloroform, and his instrument for pithing dogs, also a book which contains directions to his assistant for the preparation of lectures, in which it is provided that the animals should be pithed or rendered insensible to pain by means of chloroform or puff balls. From this book the lectures given by Dr. Pavy appear to be mostly of a physical nature. The animals alluded to are shewn only to advanced classes. In answer to questions, Dr. Pavy said that his private experimentation referred chiefly to the secretions. He said, 'I invariably use chloroform in all my private experiments, and am convinced, after much thought on the subject that, taking the aggregate of experiments required by science to be performed on animals, not one in five hundred need cause any pain whatever to the animals, and in those which I perform an anæsthetic may be administered in every case. I have told you that I do not use more than eighteen animals per term for my lectures, but I cannot say how many I use in this private laboratory. I have often said here what a boon chloroform is, as it enables us to perform almost any experiment we like upon an animal that does not quiver or start, or struggle, but allows us to cut away as we require, and does not prevent reliable deductions except in a very few cases. Although I always narcotise an animal, I do not always kill it before sensibility returns, but I nearly always do so. Sometimes it is necessary to keep it alive after the anæsthetic has ceased to act, especially in any researches relating to digestion. On one occasion I produced fistula in an animal's stomach to study the process of digestion, and kept it alive for such purpose several days; but the dog was as happy as possible, and even sat on my operating table while other dogs were being operated on without any sign of uneasiness. When animals recover from chloroform and are still kept alive in the manner described they suffer no pain. During the cutting process they are

narcotised, and after such operation, when sensation returns, I am convinced they do not suffer pain. Even human patients in our hospitals, whose limbs have been amputated while they were under the influence of an anæsthetic, feel no pain at the wound upon their return to consciousness. In all possible cases, however, I destroy the animal during its insensibility. I do not believe that students perform experiments on animals, as they have no time for laboratory work. Experiments by students would be discountenanced by every teacher at this school. I shall be glad to see you at any time, or any person you may send. If your society acts discreetly at the present time in reference to this subject, it may do much good by checking excess in experiments on living animals.' Dr. Pye-Smith also expressed himself as glad that the society was engaged in investigating into the allegations made against experimenters. His lectures were confined mainly to histology. He said, 'I have a summer course, however, when I experiment on animals, but never without chloroform having been previously administered; indeed I perform no experiments without an anæsthetic. I have done many experiments, but I am not aware that I have ever given pain to an animal. I am in favour of students seeing experiments on animals which are at the time insensible to pain. I have not seen anything performed at the Brown Institution of which I disapprove. An anæsthetic agent may in nearly every case be administered to animals before the experiment commences. I am not now referring to the trial of new medicines on animals, which is necessary before giving such drugs to human beings. Alluding to a mistake made by the memorialists respecting experiments on the eyes of animals by students, Dr. Pye-Smith admitted that it was an error arising out of the natural construction of the sentence quoted from the report of St. Thomas's Hospital.'—Signed GEORGE FLEMING. JOHN COLAM.

The Secretary attended at St. Bartholomew's Hospital by the invitation of Dr. Lauder Brunton, when he witnessed several experiments on cats and frogs performed by that gentleman in his private laboratory, in all cases the animals having previously been rendered wholly, or almost entirely insensible to pain. [I ought to say that I found Dr. Brunton extremely willing to give me every information. I cannot believe that in one or two cases the cats which I saw there were entirely without pain, not simply because of their movements, but because of the necessity there appeared to be again and again for him to insert blotting paper into chloroform, and put it on the nostrils of the animal, in order to lull it when it commenced to cry a little.] Dr. Brunton stated that such vivisections fairly represented a day's work; that each would have taken place precisely as then seen, had the Secretary not been present; and that, even if for no higher purpose, animals are invariably narcotised in this, and he believed in other laboratories, for the advantage of easier manipulation before the

experiment is commenced, and are generally destroyed before sensibility returns."

1569. (Mr. FORSTER.) Were all the animals destroyed which you saw?—They were. The dose of chloral was so heavy that I have no doubt they could not have recovered had they not been destroyed.

"The object of the experiments, which Dr. Brunton then employed was to ascertain the action on the heart of certain poisons used by the natives of Jamaica and Southern Africa for the destruction of Europeans, and to discover a remedy for the same. Two lectures delivered by Professor Ferrier at the London Institution, on the results of his experiments on animals, have been attended; from which it does not appear conclusive that an agent of insensibility is always employed by that physiologist during his experiments, although this may be done during the operation; but, on the contrary, that sometimes the animals have suffered pain. Medical journals and works published by Physiologists have been examined minutely, and many pages of manuscript extracted therefrom. These conclusively prove: (1.) That vivisectional experiments on animals are frequently used by medical men and physiologists in this country. (2.) That in many instances no anæsthetic agent is employed throughout the experiments when much pain is necessarily caused. (3.) That the alleged object of such operations is scientific research and demonstration. (4.) That the same experiment is repeated many times on different animals. (5.) That an effort is being made to instruct students, by means of handbooks, in the practice of vivisections. Evidence has been taken from persons who have witnessed vivisections at medical schools, whose testimony shows that professors seldom operate on living animals before their classes, but frequently exhibit them alive after an operation has been performed, and always while the animal is insensible to pain; that the object of such experiments, which are repeated *ad infinitum*, is solely to demonstrate facts previously established; and that students do sometimes perform experiments on living animals at their own chambers without the knowledge of their teachers. [I have a paper on this which I shall leave with you.—See p. xlii.] Dr. Hoggan, whose letter on vivisections has caused much anxiety, has appeared before us on three occasions. He cannot give any particulars of practices which prevail in this country, his experience having been confined to laboratories on the Continent. He nevertheless states that continental views on this subject and experiments are being rapidly introduced into England and Scotland, an instance being the employment of living animals to illustrate lectures in medical schools, which mode of teaching has long been the custom in Europe, but has only recently become general in the United Kingdom. The secretary of the deputation, Miss Cobbe, has been invited to supply evidence on which the allegations of the Memorial were based, and more recently the medical signers of the Memorial, about seventy in number, were addressed by letter, asking

them to be good enough to supply data respecting those vivisections of which they had complained, or to point to sources from which we could obtain the same ; but we regret to say that no new evidence has been obtained from the memorialists of a specific nature. We have also endeavoured to obtain information respecting the supply of animals to physiologists for the purpose of being used in vivisections. Much secrecy necessarily prevails on this matter, but we have discovered persons who are engaged in providing animals for the operations alluded to. These men are dealers in cats and dogs, and although naturally very reticent they have admitted taking animals to St. Bartholomew's and Guy's Hospitals for experiments, and under regulations of a private and confidential character. Vivisection is a practice in which many conflicting elements are present, and where science, acting for the prevention of human diseases and human suffering, is brought into contact with suffering entailed on a vast multitude of animals. It is therefore extremely difficult to arrive at a just determination as to measures which are necessary for the protection of animals. It becomes our duty, however, to endeavour to procure the adoption of some stringent legal restrictions of the practice of vivisection. Avoiding any extreme measure, such as the total prohibition of experiments, a medium course should be taken with a view to prevent abuses and to confine experiments within as narrow limits as possible. Repetition of operations, when deductions have already been established, should be forbidden; and unskilful and unauthorised persons should not be permitted to perform experiments. It is still more essential that anæsthesia should be insisted on whenever experiments are performed, and continued during the experiment.

The Committee [general] cannot doubt that vivisection has been considerably extended during the last few years ; and though they cannot prove that students to any extent engage in it personally, they regret that lectures are illustrated to them by animals previously operated on, and that in such matter, as in others, continental usages are being imported into this country. Such is the growing carelessness, that in one instance, perhaps unintentionally, a living lobster, they have reason to believe, has been cut up in the presence of a class of young ladies by their physiological teacher ; while popular lectures are being given of a sensational character by a learned professor, who made his audience laugh over the grim behaviour of his unfortunate victims. The evidence which is now placed before you gives proof only too abundant of the prevalence of painful experiments on animals, painless ones being left out of the extracts now submitted ; that such pain is often protracted for days and weeks ; and that a determined effort is being made to train students by the practice of experiments. Foreign experiments have been purposely omitted, except when these have been approved and recommended by English papers. Signed, JOHN COLAM, *Secretary*."

1570. (CHAIRMAN.) That is the report of a committee appointed by the society to consider the allegations of the Memorial?—Yes, with additions ordered by general committee for presentation to you.

1571. And it has been your instruction to make it known to us?—Yes.

1572. It may be taken then as the general opinion of the society that that is a document of great importance for us to consider?—Yes.

1573. You have said that a professor gave public lectures in which he sought to amuse his audience by a description of the grim behaviour of the victims of his experiments?—Yes.

1574. Who was that professor?—Professor Ferrier.

1575. Upon what evidence is the allegation founded?—Upon the evidence of newspapers which reported his lectures, and upon my own personal presence at his lectures.

1576. We may take it that you personally have heard Professor Ferrier describe the infliction of severe suffering upon the animals upon which he operated?—I have heard him say that the animals “appeared” to be in intense suffering, and then joke about the stupidity of the animal, especially if the animal happened to be a monkey, giving humorous descriptions of its behaviour; so much so that at times there was general laughter in the lecture place. Three gentlemen were with me who are members of the committee of the society, and one of them left the room in disgust.

1577. (MR. ERICHSEN.) Was that a popular lecture or a medical one?—A popular lecture. I think the place where it was given is called the London Institution in Finsbury Circus.

1578. (CHAIRMAN.) Is that lecture published?—I really do not know.

1579. Were newspaper reporters admitted to it?—Yes, I presume so, as the public could enter, and the members of the institution were admitted. I think I have got a report of similar remarks which were made at a lecture in the North. I should say the whole of the lecture, so different from anything I have heard at experimental lectures, was interspersed with parentheses of laughter. [*See p. xlvi.*]

1580. (MR. ERICHSEN.) The audience was not shocked then?—Not in the least.

1581. (MR. HUXLEY.) Professor Ferrier's experiments were experiments on the different parts of the cerebral hemispheres, were they not?—They were.

1582. He was not operating before his audience, was he?—Not at all.

1583. But he was describing to his audience the effect of the irritation of certain parts of the cerebral hemispheres?—Yes.

1584. I take it among those effects he described contortions of the face?—Yes.

1585. And contortions of the muscles of the limbs?—Yes.

1586. I presume that it may have been the case that those

contortions were in themselves very grotesque?—Yes, one might have laughed if one did not know that the poor creatures' heads had been opened.

1587. As a matter of fact, when a monkey makes contortions of the face, or moves its limbs about, it is actually very grotesque is it not?—Yes.

1588. Then the laughter which it induced may have been from the inherent grotesqueness of the thing, and not from any desire of Dr. Ferrier to make it grotesque?—Yes; but the skill of the lecturer was used to cover the grim character of the experiment by his humour.

1589. The question here is a matter of interpretation as to what Dr. Ferrier meant. I presume it is perfectly impossible for a man to describe, even with the most serious intention, the curiously grotesque grimaces that a monkey would make under these circumstances without producing a laugh?—And without laughing himself, it would appear.

1590. (CHAIRMAN.) At any rate this statement is made by you not on your own authority, but it is laid before us from official documents, and by the desire of this society?—Yes.

1591. It was on some particular day, of course, that this lecture occurred?—Yes.

1592. Can you tell us what day it was?—I should think it is six months ago.

1593. But will you furnish the Commission with the exact date?—Yes, if I have notes of it.

1594. And you can assist us in discovering whether any London newspaper contained any report of what actually took place?—Yes. I could give you also a copy, I think, of a report of a similar lecture which he delivered in the North of England, when the same laughter happened. I would like to add that I should be very sorry myself to say that there was any design to make mockery of any suffering of the animals, because, so far as I could see, there was some evidence to lead one to believe that the animal was incapable of suffering at the time when the contortions he described took place; but what did strike me, and struck a good many other people, was that as there were several young people there, and several young ladies too, it was a long way out of good taste to be making use of such remarks.

1595. (MR. FORSTER.) But am I to understand that these contortions that you are referring to were contortions of the animal under the influence of anæsthetics?—Yes, I think it was after the removal of the brain when it was alleged that the animal would be incapable of suffering in some cases.

1596. (CHAIRMAN.) But what it is important for us to understand is this, whether this is brought before us as evidence of great and unfeeling cruelty on the part of the lecturer, or merely, as what you seem now to have put it, a departure from good taste?—As a departure from good taste, and as sensational.

1597. Do I understand you on the part of the society to withdraw

that altogether in the sense of being an allegation of great and deliberate cruelty?—That was not meant for a moment. I attended those two lectures endeavouring during the whole time to discover evidence of suffering, and, with the exception of one small remark, which might be understood in two ways, it was impossible to discover whether there was any suffering on the part of the animals at all in the descriptions given.

1598. I consider this an important document put before us by the society, and I should like very much that we should thoroughly understand what we are to consider them as putting before us. Are we to consider that it is to be examined by us as an allegation made against this particular professor of doing that which I understand you to say professors generally do not do, namely, exhibiting great habitual indifference to the sufferings of animals?—No; but merely as a case of levity likely to produce a bad effect.

1599. (MR. FORSTER.) What do you mean; levity at the contortions of the animal, or levity at the contortions of the animal caused by pain?—Simply levity on the subject generally; the lecture was made what is called popular.

1600. (CHAIRMAN.) But the important thing you will observe is, that we should know whether it is an allegation against a particular professor of great indifference to the suffering of the animals?—I think not.

1601. Then for what particular purpose is it made so important a feature in a document of the Society for the Prevention of Cruelty to Animals?—Because it struck the members of the committee, as it did myself, that there was scarcely that decorum which you would expect, and that one ought really to see, in a man who was describing the condition of animals which had been mutilated by himself. There was not a word of commiseration or regret that the experiments were necessary; but there was amusement offered for the audience.

1602. But I understand you now to express belief that the animals had not suffered, but that they had been under chloroform the whole time?—He did not say; we could not gather at all whether they had been under chloroform; but I am inclined to think, from the nature of the experiments, it was absolutely necessary that they should be.

1603. Do you mean us, as far as you can guide us, to understand that these animals had been subjected to great suffering, or that, whatever had been done to them had been done under complete anæsthesia?—I would rather be inclined to think that the animals did not suffer, judging from the description given of the experiments, and from what I have read myself in the various books which I have been obliged to read. The allusion to the experiments of Dr. Ferrier in the paper which I have read, where there is an intimation that sometimes there is suffering, is to cases where he states that the animals have remained under the operation for several days.

1604. (MR. FORSTER.) I understood you to say that you thought

the reason why there was no suffering was because the brain had been removed?—Yes, after the operation had been completed the effect of the narcotic would cease in most cases before the experiment had been concluded.

1605. Do you think that it was that there was no suffering because chloroform or some anæsthetic had been administered, or because the brain had been removed?—Of course we did not see the experiment, we only heard a description of it, and I should think that chloroform must have been used during the operation. I am inclined to think it would kill the animal to perform the operation without chloroform, the effect of which would last until perhaps after the brain has been removed, partly or wholly; therefore, in the first part of the experiment there would be no feeling, owing to the presence of a narcotic, and, in the second part of the experiment, owing to the removal of the brain.

1606. (MR. HUTTON.) May I ask you to explain your statement that the lecturer joked about the stupidity of the monkey. Those movements were involuntary, were they not?—Of course they would be, because he showed that the animal had no volition at all.

1607. And therefore it was meant as a sort of joke upon the apparent expressions of the monkey?—Yes, and the loss of intelligence of the animal.

1608. (CHAIRMAN.) Have you any record at the Society, any newspapers filed that contain a record of this lecture?—I do not think that a report of the lecture appeared in any London newspaper. Probably newspaper reporters do not attend at the London Institution on such occasions, in which case there would be no report of it. Two barristers were with me, one Mr. Robert Sawyer, and the other Mr. Thomas Allen, and another member of the Committee, Mr. Thomson, and one of those three gentlemen left the room in consequence of the pain with which he saw the laughter of the young people.

1609. (MR. FORSTER.) I thought you stated that there were reporters there?—I did not see any reporters there.

1610. But you stated something about reports of the lectures appearing in the newspapers?—That was in the North of England.

1611. (MR. ERICHSEN.) Did not this lecture consist of a description of the movements induced in an animal by stimulating certain portions of the surface of the brain by electricity?—Yes, in some cases.

1612. And consequently the brain could not have been removed?—I cannot pretend to be very accurate in these matters, except with regard to the impression produced on our minds by what we heard. I think the skull was only opened in some cases, and in others the brain was also partly removed; but I would rather not answer any questions in detail about that as the experiments were not made before us but only described.

1613. The contortions of the animal in that case were simply the natural movements of the animal brought into operation by the stimulation of certain portions of the brain?—In some cases.

1614. (MR. FORSTER.) You mentioned another case, that of a living lobster being cut up before a ladies' school; could you give us any more particulars about that case?—It was a lady who was the teacher, who had commenced a course of physiological lectures at a school which is about twenty miles from London. The complaint was sent to us in the ordinary way, under a pledge of secrecy; that is to say, of course the complainant did not wish to be known. It had been told by one child to its parent, and the parent communicated it to us, and we caused inquiry to be made. I sent down one of my assistants, (I did not send an officer down; under the circumstances I decided not to do so), and he saw the lady to whom the school belongs, and he learned that the facts were—that the teacher did take a lobster and put it upon the table, and then cut it, and after one portion of the lobster had been severed, there was seen something more than "muscular action," for the severed part of the lobster crawled upon the table.

1615. Was any chloroform or other anæsthetic administered?—No. The explanation given by the lady is I believe that she bought the lobster on her way there, and did not know that it was alive; and I think that is likely to be the case; only, if so, I think she was a bad physiologist.

1616. (MR. HUXLEY.) The shortest and best way to kill a lobster is to cut into its pericardium, and she may have done that as the best way of killing the thing without any bad intention whatever, may she not?—I am told that she concluded that it was dead.

1617. (MR. FORSTER.) But I think in the mention of this case in the report you make no allusion to that explanation; it is said that it was carelessness?—There is no desire to misrepresent the matter, and we are really under a pledge of secrecy not to divulge the name. These are the words of the report: "such is the growing carelessness that in one instance, perhaps unintentionally, a living lobster, they have reason to believe, has been cut up in the presence of a class of young ladies by their physiological teacher." The description given to us by the lady of the school justifies our statement.

1618. (SIR JOHN KARSLAKE.) Had she heard it from the little girl?—She was present when it was done, and was horrified by it.

1619. (MR. FORSTER.) With regard to this lady lecturer, I do not wish to ask her name, but was she a teacher in the school?—No; a physiological teacher.

1620. (MR. ERICHSEN.) Was she a medical practitioner may I ask?—I think not.

1621. (CHAIRMAN.) Is it possible to imagine that a lady could buy a lobster, carry it in a cab, put it on a table, and keep it on the table,

and then proceed to cut it up, and then for it to crawl about afterwards, and that she did not know during the interval whether it was alive or not?—The lady had in her bag at the same time a dead rabbit on which she experimented before the class, and she is in the habit still of doing the same thing; and I believe that it is her explanation, that she thought the lobster was dead; and I think it is consistent. I think Professor Huxley will agree that so far as we are concerned we are not guilty of any misrepresentation; we have simply used the words put into our mouth, that it was cut up,—in what way we cannot say,—except that there was one portion severed from the body, which did move, and which horrified the people there. To tell the truth, the lady of the school was extremely anxious that no further inquiry should take place lest it should do damage to the school.

1622. (SIR JOHN KARSLAKE.) There was a portion of the report of your society in which reference is made to the growing practice amongst students in some parts of the country of carrying on vivisection; will you just read that portion again?—I think there is an allusion to the circumstance that evidence had been obtained in one case of experiments by students at Edinburgh (as is shown in the paper which I hold in my hand), which experiments were performed by them in their private rooms; but it is qualified here by saying “unknown to their teachers.”

1623. Will you read the paragraph of the report in which that is referred to?—I will read from the report an allusion to students being taught vivisections, which is as follows:—“A determined effort is being made to train students in physiology by the practice of experiments.”

1624. On what foundation does that statement rest?—On several facts. In the first place, a handbook has been published, which is designed for no other purpose than to teach beginners, the preface of which declares that it is for beginners, which gives sixty-three experiments involving pain, and gives a full and detailed account of every instrument that is required for experimentation in order that it may be as plain as possible to the mind of a beginner. This book says the experiments may be repeated any number of times. [See p. 95.]

1625. What is the name of that book?—*The Physiological Handbook*, edited by Dr. Burdon-Sanderson, in which papers appear by Dr. Klein, Dr. Burdon-Sanderson, and Dr. Michael Foster.

1626. Is it upon the fact of that handbook having been published that that paragraph finds its way into the report?—That is part of the reason.

1627. What is the other part?—The other part is that at various meetings which have been held, teachers of physiology have insisted not only upon the desirability, but upon the duty of bringing students into laboratories, and instructing them in laboratories by experiments upon animals performed in the presence of the students. Dr. Rutherford is an instance. [See p. 96.]

1628. What other gentleman can you mention as an advocate for that practice?—Dr. Brunton also told me that if the students were not taught to manipulate and to experiment, the next generation would be found without any experimenters, and therefore it was desirable that they should be taught. So far as he was concerned, he thought it was right that they should be taught the experiments.

1629. Did that apply, as you understood it, to all students or to a certain class of students?—To all students who wish to become physiologists; but all medical students are required now to study “practical physiology.”

1630. And did he limit at all the cases in which he proposed that these experiments should be made before them?—No.

1631. He did not define them at all to you?—No.

1632. Now, in your own personal experience which you say has existed for about fifteen years, have you known instances yourself where cruelty has been practised by private medical men in their own houses towards animals?—Not any.

1633. Or as far as you know, has that come to the knowledge of the officers of the society?—As far as I know, it has not.

1634. I ask that question because, as I understand the Bill which the society has laid upon the table here, it is proposed in the Bill that, in the event of reasonable suspicion attaching to a particular person, a justice of the peace might issue a search warrant?—When it becomes necessary to obtain a license, and when persons have to go through a form to get that license there may probably be a temptation to some persons to perform experiments without really having been properly qualified. This will account for that provision.

1635. With regard to that proposal in the Bill which you have laid before us, is there any instance that you know of in the course of your experience during the last fifteen years, or any instance reported to you by your officers, in which you would have put that section into force?—Yes; cases have often been reported to me as rumoured.

1636. Was it mere rumour, or was it founded upon any evidence at all in those cases?—Not evidence exactly—strong suspicion, perhaps; not conclusive but *prima facie* evidence. Your question was with regard to medical men. Of course medical practitioners, as a rule do not perform many experiments.

1637. Now, I understand you to say that there were a class of men who were known to your society who supply animals to the different laboratories or schools?—Yes.

1638. From whom did you get that information?—We got information by watching.

1639. Now, may I ask you ‘this. I think you said that there were certain regulations under which those men supplied the animals to those who employed them?—Yes.

1640. From whom did you hear that there were certain regulations?—From the men themselves; the officers of the society report that to me.

1641. Can you tell us any hospital to which it is alleged animals are supplied, and where particular regulations are issued to the suppliers of those animals as to the mode in which they are to be supplied to the hospital?—Yes, certainly; St. Bartholomew's.

1642. Who would be the person who would probably have to give those instructions if they exist?—The porter at the gate.

1643. And I suppose he would act under the physiologist or some person there who has the conduct of the experiments?—Yes; Dr. Legge or Dr. Brunton I should think.

1644. (MR. HUXLEY.) With regard to the case of the lobster which you mentioned, I suppose you know what commonly happens to lobsters?—With regard to boiling them, I suppose you mean; yes.

1645. Does the society ever interfere on that point?—I have been down myself to see them at Billingsgate put into the water, and they die instantly.

1646. But they are put into boiling water?—Yes; I have been there several times in the middle of the night for the purpose of seeing it done and have found no cruelty.

1647. I should like to ask you a few questions as representative of the society, as to what you conceive would be the effect of the Bill now before us supposing it to become law. There is a famous experiment which was made a number of years ago on which a great deal of our present knowledge of physiology of the nervous system is based, called Bell's experiments on the roots of the nerves. It cannot be performed without the infliction of pain. Would the Bill, if it became law, stop an experiment of that character?—I scarcely know the nature of the experiment, not being an expert.

1648. I take it for granted that the experiment is a very painful one, and cannot be otherwise. The Bill would stop it, would it not?—Yes; but you must remember that Sir Charles Bell said about that particular experiment that he merely performed it for the purpose of convincing people who did not use inductive reasoning, and Dr. Ebenezer Watson, himself a vivisectionist, the other day publicly stated that it was the greatest satisfaction to his mind to know that that great discovery was established by only one experiment on one animal, and that experiment was for demonstration.

1649. Suppose that an experiment having a similar value in relation to physiology needed to be performed, would the Bill, if it became law, prevent its being performed?—As in our Acts of Parliament magistrates have to construe what cruelty is, and as now there are some things which might certainly be considered cruelty which no magistrate in the world would convict for, so magistrates might be left to construe what pain is under the new statute.

1650. Does this Bill of yours leave any discretion to magistrates?—I must admit there is no room left for much discretion. Such animal, it says, is to be submitted to the influence of an anæsthetic, so as to render it *wholly* insensible to pain.

1651. The Bill says that the animal is to be wholly insensible during the time that the experiment is being performed, does it not?—Yes.

1652. In that case a magistrate would have no discretion whatever, would he?—A magistrate would have the discretion certainly of dismissing the case if he chose.

1653. That is to say, if he thought it could not be proved that there was pain?—Yes.

1654. But I am now supposing that to be admitted. I am supposing the case to come before the magistrate in this way, that it is admitted that as much pain was given as would be given in that particular case I have mentioned, would your Bill prohibit it?—I admit it would. I do not wish to be uncandid.

1655. I understood you to mention with reprobation the experiments made by the Medical and Chirurgical Society on dogs in order to ascertain whether the methods of restoring life in cases of suspended animation could be improved. Therefore, I presume that this Bill, if it becomes law, would stop all such experiments as those?—I hope so.

1656. Can you tell me whether the Bill, if it became law, would have the effect of stopping such experiments as those which have been recently made by Dr. Klein, and which have thrown such very great light on the nature of small-pox; that is to say, sheep have been inoculated with sheep-pox, and the whole course of the disease has been studied in sheep. No doubt that gave the sheep a certain amount of pain; now would the effect of the Bill be to stop all experiments of that kind?—I really am not competent to say whether the experiment would in that case be considered pain, or whether it could be performed with an anæsthetic.

1657. Undoubtedly sheep-pox is a painful disease. I am supposing that to be admitted?—That is hypothesis I think.

1658. There is as much reason to suppose that a sheep is uneasy when it has sheep-pox as that a man is uneasy when he has small-pox; and we know perfectly well that a man is uneasy when he has small-pox. Now, supposing that Dr. Klein, or any such person, were engaged in experimenting in that way, under your Bill, if it became law, it would be possible for a person to lay an information against him, and he might be dragged before a magistrate and charged with committing a breach of the law. I wish to ask whether that would be a breach of the law in your judgment?—As the representative of the society I must say yes, if painful.

1659. About a hundred years ago there was a French Abbé, named Trembley, who made a number of experiments upon what is called the hydra, a fresh water polyp; he made transverse sections and longitudinal sections, and all sorts of things, and he discovered, what was a very wonderful fact, and one which effected a great revolution in the way of thinking in those days, that each of

these things would grow up into a new polyp. Suppose a person were to repeat those experiments in the present day, or anything like them, in your judgment, would he come under the operation of this Bill if it became law?—I must answer in the same way, that assuming that there is pain he would ; but I am not inclined to say there was pain inflicted in that case.

1660. There is no limitation in your Bill as to the kind of animal, or as to how much pain is inflicted?—No.

1661. One of the commonest things shown in a class is the circulation of the blood in a frog's foot. To show that, you have to tie the frog's toes out, and otherwise make him more or less uncomfortable, and then you see the circulation, which is a very wonderful sight. Should I, as a teacher of physiology be prohibited from performing that experiment if this Bill became law?—I should think not. I should think that would scarcely be called pain.

1662. I beg your pardon, I should admit at once that there was pain there?—Speaking microscopically it would be pain I suppose.

1663. I want to know what physiological people are to expect if the Bill becomes law?—At the present time you must remember there is an Act of Parliament which provides that you shall not ill-treat or torture an animal ; and in applying the law therefore at the present time it has to be determined what is torturing or ill-treating an animal. It might be contended in a given case that there had been ill-treatment or torture in a very small degree, and thus, if Dr. Klein's sheep were tortured no more even than your frog in the case you have put, you might possibly contend that now under the present Act we could take Dr. Klein before a magistrate ; but you must remember that no society, no prosecutor, can ever go beyond public opinion, and therefore it is idle to talk about infinitesimal cases of that kind, I submit.

1664. I think you said that you thought it would be desirable that the Bill should give the society power to test whether experiments were doing any good?—I said that the object would be two-fold ; the first advantage would be that it would enable the society and every person else to judge, from the register of experiments performed and the results which had been achieved, whether the experiments were of any value at all. Secondly, it would prevent a repetition of experiments, because the publication of the experiments would go to the ends of the earth, and prevent any unnecessary repetition of experimentation.

1665. Then am I to gather that you think that the Society for the Prevention of Cruelty to Animals is a fit high court to which to refer scientific inquiry, to judge whether a particular class of experiments is doing any good or not?—I beg your pardon, I did not mean anything of the kind.

1666. I ask the question because that is one construction which your words would bear?—I did not mean that ; I meant this, that all

persons would be enabled to form a judgment, and I suppose members of the Royal Society for the Prevention of Cruelty to Animals can form a judgment as well as those who are not members of that society, some of whom are very well able to form a judgment scientifically. It would enable them as well as other members of society at large to form a judgment on that question.

1667. But in the long run it would constitute the society judges of the scientific value of a given result :—Only as we are judges now of anything ; judges, for instance, of the operation of any Bill that may be brought in during the next session of Parliament.

1668. (MR. FORSTER.) You mean that it would give publicity which would enable you to form an opinion of the value of the experiments ?—Yes.

1669. (MR. HUXLEY.) But I do not see how any but experts can form any opinion as to their value. I want to know whether that is one object of the Bill to constitute the Society for the Prevention of Cruelty to Animals a sort of court of revision in these matters ?—Do you not think, I may ask, the society would be able to judge whether for instance the experiments referred to by you had prevented sheep disease or cattle diseases or not, if those diseases disappeared by the application of the remedies discovered by the experimenters ?

1670. No, I think not. I think that if the society contented themselves with reading Dr. Klein's memoir, recently published, unless I am much mistaken, there is not one in five hundred who would understand anything about it ?—Not the technicalities ; but the general results we might understand. I hope so, else I think the experiments would be no great advantage to humanity.

1671. (CHAIRMAN) What I understand you to say is that you wish this Bill of yours to be understood as subject to reasonable interpretation like all other Bills for the prevention of cruelty ?—Of course.

1672. And if the terms of it are such as to exclude a reasonable man in the chair of the magistrates from determining whether there is cruelty in the particular case, you do not think the society would insist upon those particular words ?—I think not.

1673. Then the only purpose for which the society are to be judges is whether it is a case in which they ought to institute a prosecution ?—That is so.

1674. And then it would be for the court to determine whether they had sustained that prosecution ?—Yes.

1675. And experts might be brought forward for the defence if it was desirable that the evidence of experts should be heard. Is that what you wish us to understand ?—Yes, as in the case of the Norwich prosecution. We had there doctors against us and doctors for us, and the contention was then, as it would be under our Bill,—was pain given or not ?

1676. The phraseology of the Bill as you have laid it before us, does not seem to some of us to admit of that reasonable interpretation,

but seems to be too precise, that the court must necessarily act upon the mere words without taking the reasonableness of the thing into consideration at all. If such should be the true legal interpretation of this Bill, are you authorised on the part of the society to say that they would wish the words to be qualified?—The society consists of practical men and reasonable men. In discussing this Bill before the Government I am obliged to state that it would be impossible for the society to move from their position.

1677. But you have now been asked whether, if your Bill were to pass through Parliament in the shape in which you have submitted it, such and such consequences would not follow; and you have said that in all other cases, and particularly you say in the Norwich case, the reasonableness of the conclusion is submitted to the judgment of the court; would you take upon yourself to say that the society really wish that in any bill which should be framed in obedience to their suggestions, the reasonableness in this case also should be taken into consideration?—Yes; of course they would be very glad to have the most reasonable phraseology used.

1678. (MR. FORSTER.) I want to ask you a question following upon Professor Huxley's questions, not so much as regards the actual wording of the Bill, but as to the intention of the committee of the society with regard to legislation, so far as you can give it. Now it has been stated to us in evidence by Mr. Simon that he considers that by giving animals a disease, and I imagine a painful disease, he obtained information with regard to cholera, which he considers is of very great service in sanitary legislation, and in enabling an Act of Parliament to be passed which will very likely diminish cholera. Now if this Bill had been passed, or if any bill had been passed such as your society wishes before he tried such experiments, would they or would they not have desired that he should have been prevented by law from trying them?—I think if it were any very severe infliction of pain on animals, perhaps the society would like to have prevented that. But the society has come to the conclusion, from reading the history of vivisection, and the history of experimentation, that one generation of experimenters arise for the purpose of correcting other experimenters, and that each generation of experimenters has really some panacea which turns out afterwards to be of no value at all.

1679. My question was not with regard to vivisection, but with regard to the giving of disease. What I wanted to know was the exact desire of the society. Do you think that the object of that committee of the society which has framed this Bill would be to prevent such an experiment as that, and that in fact the law which they would wish passed would have prevented that if it had been passed?—I think so if it were a very severe act of cruelty.

1680. I have interpreted it to be the giving of a disease?—A disease might be given, I apprehend, without much pain being caused to the animal.

1681. But before you would have allowed that thing to have been done you would have required, not merely to have been satisfied that there was a reasonable probability that it might result in the prevention of cholera, or do something towards the prevention of cholera, but you would also have wished it to be proved that there would not be severe suffering caused to the animal upon whom the experiment was tried?—I am quite sure that the society would not prosecute (although they might desire the law) in any case, such as you are suggesting, and I say that from the course which is taken now very frequently. For instance, in a case where a horse had been over-ridden to save a life.

1682. (MR. HUTTON.) Have you handed in the evidence of the cases of experiments by students in Edinburgh, to which you have referred in your written statement?—Yes, I have.

1683. You have nothing to add to what is written there?—No; except that in regard to London I have not been able to find a single case where the students have been employed as vivisectors.

1684. Are the names of the students given and the nature of the experiments in the paper which you have handed in?—Yes, of my informant.

1685. (CHAIRMAN.) Will you read the paper you have with regard to experiments made by students?—Yes. This is a statement made by a student of Edinburgh and since Lecturer at the New Veterinary College, Edinburgh, on *Materia Medica*, who saw Mr. Ernest Hart's letter in the newspaper and wrote to us in consequence.

1686. And by the desire of the society it is laid before us?—Yes.

1687. (MR. HUXLEY.) What is the date of it?—I think it is about six months ago. "Observing from the daily papers that Mr. Ernest Hart alleges that students do not perform experiments on living animals as an exercise in the prosecution of their studies, I beg to forward to you a summary of my experience in that respect during my college career at Edinburgh. I am a Veterinary Surgeon, and comparatively unknown, but I feel it my duty to aid your society in repressing unnecessary experimentation, surveying the past as I do with much regret, so far as I have participated in the practices which I am now compelled to condemn. At Edinburgh the veterinary students and the medical students frequently associate for pleasure and for study. During my first term I was admitted only to two private meetings where experiments were conducted by students alone; but in the following term, having become a senior, I was introduced to a great number of such vivisections, and on some occasions operated myself. The experiments were certainly never designed to discover any new fact, to elucidate any obscure phenomena, but simply to demonstrate the most ordinary facts of physiology. Our victims were sometimes dogs, but more frequently cats. Many of the latter were caught by means of a poisoned bait, the animal being secured whilst suffering from the agonies caused by the

poison, when antidotes were applied for their restoration. They were then imprisoned in a cupboard at the students' lodgings, and kept there until a meeting could be arranged. Sometimes the students secured their victims by what is known as a cat hunt, that is a raid on cats by students armed with sticks late at night. I am not prepared to say that the object of the students was to commit cruelty, or that there was any morbid desire to witness pain, but I say emphatically that there was no other motive than idle curiosity and heedless, reckless love of experimentation. What, for instance, could justify the following experiment, performed for the purpose of witnessing the action of a cat's heart? The operator first of all made an incision through the skin of the animal's chest extending from the neck to the belly. The skin was then laid back by hooks, in order to enable the operator to cut through the cartilage of the breast bone, and to draw his knife across the ribs for the purpose of nicking them. This process is necessary to enable him to snap the ribs and lay the fractured parts back, which also are secured with hooks. It is needless to say that such operation is a most cruel one; but it is only one of several others performed at Edinburgh. Now, the action of the heart is well known, and is one of the first things taught to students of physiology, and can be taught as well without experimentation as with. In a few cases the animals were narcotised, when no suffering was caused either in the process of poisoning or in the after experimentations. The securing an animal for an operation like the above requires experience and care, and it is fearful to witness the struggles of the animal while this is being done. I desire to exonerate the professors from any participation in the experiments performed by students which were conducted at the private lodgings of students, when none but students were present. I merely write this in order to give my humble corroboration of the statement made in the memorial, that students are in the habit of performing experiments. Jas. B. Mills, M.R.C.V.S." The writer of that letter is a Veterinary Surgeon at the present time in the Royal Artillery at Woolwich.

1688. He does not state whether that particular experiment which he describes with so much detail was made under the influence of narcotics or not?—No, he does not, except by saying that it was a most cruel experiment.

1689. (MR. ERICHSEN.) I should like to put a question in reference to what came out in the evidence of a very eminent surgeon at the commencement of this inquiry, with regard to the provision in the proposed Act by which it becomes incumbent to administer anæsthesia in all painful experiments. An inquiry was mentioned to us as of the very utmost possible practical character, which is necessarily attended with considerable pain, and that is in the discovery of an antidote to the snake poison; there are at least 10,000 people who die every year in India from snake bite; and there is

no possibility of arriving at any knowledge of an antidote for snake bite, and consequently no hope of saving the lives of those people, except by submitting animals to be bitten by snakes and then testing the power of the alleged antidote upon those animals. That I imagine, would be a very painful experiment to the animal, and it would be impossible to perform that experiment under anæsthesia, and yet the result of such an inquiry might have been of inestimable service to humanity and a direct service because it is not a scientific but a purely practical inquiry. Would the provisions of the Bill interfere with such an inquiry or investigation?—I presume they would, but at the same time I would say that Sir William Fergusson another eminent surgeon has expressed a very strong opinion that these experiments are entirely useless.

1690. No result has as yet been arrived at, no antidote has yet been found, but the only hope of discovering an antidote, as Sir James Paget told us, in the next twenty years, would be by continuing experiments of that kind on animals. Is it your opinion that the Bill, as proposed, would interfere with the performance of such experiments, which would be of the most direct practical benefit to mankind, and would, if successful, save the lives of thousands annually?—It would be of value to mankind if it would save lives; but that is the whole question I presume. If it could be established at once that an experiment would yield a blessing to mankind the result would be already known, and there need be no experimentation at all; it is the uncertainty of the thing that requires experimentation. I therefore would not admit the premiss that the experiment would necessarily be of the most direct practical benefit to mankind.

1691. (CHAIRMAN.) But the question put to you is whether your Bill would prevent the process which the Government of India are now carrying on for the purpose of preventing the loss of life by snake bite?—I am afraid it would.

1692. Take another case; we have been told that the condemnation of Palmer for one of those numerous murders, and the condemnation of other criminals, has been very much due to certain experiments made upon animals by the use of strychnine or other poisons; would your Bill prevent that?—I am afraid it would. But at the same time, I would like to say that it is a very difficult thing to answer these questions in the way I am doing now, because I have in my mind, at the same time, the statements made by a man like Sir Charles Bell, who says that experimentation has not yielded the results claimed for it. I am not going to say that experimentation has not yielded anything; at the same time, I cannot allow it to be assumed that the experiment is going to answer.

1693. But we are assuming nothing, except that it is the object of the experiment to discover a cure for snake bites, or to bring a murderer to justice. Would your Bill prevent an experiment as to

the effect of strychnine on an animal, or subjecting an animal to a snake bite?—I should say so. But I am sure Sir John Karslake will tell you that there are many things which you must not do even to bring a murderer to justice. Although it would be a good thing to bring a murderer to justice by certain short cuts the practice of the courts does not admit of them, because the end does not justify the use of improper means.

1694. I want you to understand that the object of the questions which have recently been put to you, and are now being put to you is not to argue it one way or the other, but merely to ascertain whether the effect of this Bill if passed in the sense in which it is submitted to us by the society, would be to render such experiments impossible?—I think so.

1695. However strong the scientific opinion in favour of the experiment might be, do you think that those who have submitted that Bill to us, would wish the Bill to pass in a form which should render the experiment impossible?—I think that they would like to have it passed in this form, and if that is not possible, then to get it as near as possible to this form—if at all.

1696. (MR. FORSTER.) Do you think that they have had before them at all, this case of snake poisons?—Yes, I think those cases have been before the committee.

1697. Could you tell us whether it occurred to them that there was any other way of finding out whether a supposed antidote was likely to be an antidote, than by giving an animal poison, and then the antidote, to see whether it cured it or not?—There are many ways perhaps. I have seen experiments to test the action of poisons upon dead animals, upon the hearts of frogs, for instance. The heart has been taken out from the body of the animal, when the pulsation continues for a long time; and I have seen the poison tested in that way without any cruelty at all. I do not know that that would be applicable in all cases, but there may be some other means by which the want would be met. It may be a more easy and a more rapid way of doing the thing, and one that would prevent a good deal of thought and study, to experiment on a living animal; but the question is, is it right? I do not say absolutely that it could be arrived at in any other way, but I say it is possible, it might be; but it would be better to kill the snakes than to allow them to go on biting people and then provide antidotes by killing and torturing other animals.

1698. (MR. HUXLEY.) Did I rightly understand you to say that the society, in drawing up this Bill, have been actuated, among other things, by a firm belief that physiological experimentation has not done much towards the discovery of new truth?—I think that is the general impression of the society as regards the treatment of disease.

1699. On whose authority may I ask?—When I speak of the society I am speaking of the committee rather, the society itself consists of many members.

1700. Is there any one in the committee who is conversant with the history of physiological science in the last twenty years?—Yes, I think so. There is one member of the committee who is a doctor of medicine, and has had a good practice at the West End, who says he does not believe vivisection has done any good to his profession; and another member has made the subject a special study, is the author of several medical books, and the editor of a journal which includes physiology and anatomy entitled *The Veterinary Journal*.

1701. (CHAIRMAN.) In giving in the documents connected with the committee, have you given the names of the committee?—No, I have not.

1702. Will you do so?—Yes.

The witness withdrew. He handed in the following books and papers:—"Vivisection: Is it necessary or justifiable?" being two prize essays, by George Fleming, Esq., and Dr. Markham; "Vivisection Prosecution," being a report of the Norwich prosecution by the R.S.P.C.A.; "Handbook for the Physiological Laboratory," 2 vols., edited by Dr. J. Burdon-Sanderson; "The Queen's Letter" on Cruelty to Animals, addressed to the R.S.P.C.A.; "Notes on Plans on Edinburgh University Extension," by Mr. Anderson; Draft Bill for a statute to prohibit painful experiments on animals, and minor papers. He subsequently forwarded the following letter to the Royal Commission in vindication of his evidence on Dr. Ferrier's lectures with newspaper extract on next page, as desired by the Commissioners:—

*Institution Building, 105, Fennyn Street, St. James's, S. W.
18th December, 1875.*

SIR,—I beg to send you slips of the newspaper report referred to in my evidence, which I promised the Commissioners to lay before them. It has been printed from the file copy of the *Leeds Times*. *The Mercury*, and other papers correspond in giving five parentheses of "laughter." You will remember that the slips refer to meetings in Yorkshire—not to those I attended in London.

Permit me to observe that (1) there does appear very little to laugh at in the report of Professor Ferrier's remarks, from which you may infer that there is much force in my observation, that the lecturer made the jokes by gestures as well as by what he said;—(2) there is nevertheless sufficient proof in the slips that in Yorkshire, jokes were attempted.

At the London Institution we heard allusions to *Pickwick*, besides several others we have remembered, intended to amuse the audience, the popular element being prominent—as I said in my evidence.

I have spoken to Mr. Allen and Mr. Thomson (members of the Committee who were present) and they confirm my remarks generally; as do also two other gentlemen present on the occasion.

I am, yours faithfully,

To N. BAKER, Esq., Secretary,

JOHN COLAM.

Royal Commission on Vivisection.

Extract from the LEEDS TIMES, March 1st, 1874, referred to on pp. xxx. and xlv.

THE FUNCTIONS OF THE BRAIN.

On Tuesday night Professor Ferrier delivered a lecture at the Leeds Philosophical Hall on the "Localisation of the Functions of the Brain." Owing to the interest of the subject, and the striking originality of the experiments made by Dr. Ferrier in his investigations, there was a crowded attendance, including a large number of medical gentlemen.—Professor Ferrier said, that at the British Association meeting in Bradford last September, he brought forward the facts of experimental research carried on by a new method, which established to a certain degree the localisation of certain functions of the brain. More recent experiments still further substantiated this doctrine, but some physiologists still objected to it, maintaining that the brain in its entirety was subservient to mental work, and that no part of it more than another was allotted to certain specific functions. Upon this point his experiments would speak for themselves. Other critics had taken exception to his method of experimenting upon live animals. Upon this point he explained that the brain, unlike all the rest of the nervous system, was curiously insensible to irritation, except by electricity. About a couple of years ago two German philosophers, experimenting on the brains of dogs, found that certain muscular movements could be produced by touching various parts of the brain, and he (Dr. Ferrier), following up their researches, had taken up the subject in the way in which he would now proceed to describe. The animals experimented upon were first rendered insensible by chloroform, so that the experiments were carried on in such a way as not to offend the scruples of the most rigid humanitarian. An opening having been made in the skull and the brain freely exposed, the animal was allowed to recover itself from the effects of the chloroform. The mere operation on the brain apparently produced not the slightest effect on the health of the subject. Often he had seen a monkey wake up after several hours' experiment and enjoy a cup of tea with bread and butter just as heartily as any human being. (*Laughter.*) It was quite a mistake to suppose that if the substance of the brain was touched instantaneous death followed. The lecturer proceeded to describe how the application of electricity to the surface of the brain gave rise to certain distinct movements of the body. The remarkable fact was, that the same result invariably followed the application of the electrical stimulus to the same part; so that, having thoroughly explored and mapped out a brain, he could undertake to predict with absolute certainty what an animal would do upon the application of the electric stimulus to certain parts of its brain. The effect was somewhat like playing upon the keys of a piano; it was playing upon the animal's powers independently of its will. By means of diagrams, Dr. Ferrier showed the brains of man and various animals, including the monkey, dog, cat, rabbit, pigeon, frog, fish, &c., explaining that in each case the brain consisted of two parts known as the cerebral hemispheres. Except in the case of the rabbit and pigeon, the brain of most of these animals was raised into a number of irregular folds or convolutions; and it was a singular fact that we could trace the same homologous folding from animal to animal down in the scale until we came to the nonconvoluting animals. In the monkey's brain the convolutions were of the same type as in the human brain. The cerebral hemispheres had cross action and cross influences, the left side of the brain governing the right side of the body, and *vice versa*. Dr. Ferrier proceeded to describe with some detail how, by touching certain points in the brain, he could cause an animal to make certain movements with its body, such as advancing its hind or fore legs, opening its eyes widely, and staring as if in surprise, moving its lips or tongue, &c. In a similar way he could make a dog bark and a cat mew; so he could make a dog, or a monkey, or a rat wag its tail; but added the lecturer, amid some amusement, he could never get a cat to wag its tail. From such experiments he could trace, for instance, where the faculty of articulation was centralised, confirmed by the experience that where that part of the brain was affected by disease the human patient lost the power of speech. He had not been able to obtain many results by stimulation in that front part of the brain which was supposed by phrenologists to be associated with all the higher powers of the intellect. In order to determine on this point, he experimented on a clever monkey, which he had kept a long time so as to learn its habits. It usually exhibited a great amount of natural curiosity, taking the strongest interest in everything that was shown it. (*A laugh.*) He cut off the fore part of its brain,

and it got quite well again, perfectly well, shortly afterwards. (*Laughter.*) Interference with the structure of the brain, be it remembered, did not interfere with the general health, and therefore the animal went on eating and drinking after the operation, but it soon became perfectly stupid, perfectly demented. Even when eating it looked as if it were sound asleep, like Dickens's fat boy. (*A laugh.*) Whenever it was nudged, it just renewed its eating or climbed up into its cage, and so dozed away. It had not lost any of its physical powers, the only change being its excessive stupidity. Singular cases had occurred where persons recovered all their faculties after parts of their brain had been injured; but those who adduced this as an argument against the localisation doctrine forgot that the brain was a double organ, one hemisphere continuing to act when the other was more or less destroyed, and even taking up in time some of the functions originally belonging to the other hemisphere. Resuming an account of his experiments, the lecturer traced where the senses of sight and smell were localised in the brain, showed that there was an extra large convolution for the olfactory nerve, in the case of animals which, like dogs, had a keen scent. Definite as were many of the results thus ascertained, it would be useless to rush from these experiments to speculations, seeking to evolve all the generation of mind and intelligence. It had been supposed by some phenologists that irritation of a certain part of an animal's brain excited a mental perception under the influence of which it performed a corresponding action, but this doctrine would not account for the results of the experiments he had made. Such a movement, for instance, as that of stretching out the hand or fore foot and paw might be prompted either by "acquisitiveness," if for the purpose of seizing something, or by, "benevolence" (*A laugh*)—if in the act of giving something. It therefore seemed impossible to localise ideas when the same bodily actions might be stimulated by widely different ideas. In the cerebral convolutions the mental furniture of a man might be stored up, and a high development of certain parts of the brain might nevertheless suggest certain conclusions, but the inferences so drawn must be very hypothetical.—The Chairman expressed the cordial thanks of the society for the lecture, and his high sense of the interest and importance of Dr. Ferrier's investigations. There had not yet been time for the attainment of great practical results, but they must all hail every new addition to our stock of scientific knowledge, especially on so important a topic as the function of the brain.—Dr. Clifford Allbutt also expressed his appreciation of the importance of Dr. Ferrier's experimental researches into this great subject. He added a word of satisfaction that the West Riding was to some extent associated with this original investigation, which had been largely conducted at the Wakefield Asylum, was first expounded in public at the British Association meeting in Bradford, and was now renewed with the result of fresh experiments at Leeds. He was glad that Dr. Ferrier had not confined his experiments to dogs and monkeys, but had communicated some electric stimulus to the minds of his audience—(*laughter, and hear, hear*)—upon whom he had no doubt the lecture would leave a deep impression.—The meeting then separated.

The Secretary of the R.S.P.C.A. suggested to the Royal Commission that the following witnesses should be called:—Sir William Fergusson, Professor Pritchard, Dr. Arthur de Noé Walker, Dr. Lawson Cape, Mr. Macilwain, the Rev. Samuel Haughton. M.D., D.D. Dr. Hoggan, Mr. Jesse, Mr. James B. Mills, Dr. W. B. Archibald Scott, Mr Goodson (Secretary of the Birmingham S.P.C.A.), Mr. Torr (Secretary of the Nottingham Branch of R.S.P.C.A.), Mr. Robert Brewin (Secretary of the Cirencester S.P.C.A.), Dr. Edward Curtis May, and about sixty medical men who signed the memorial presented to the R.S.P.C.A. Sir W. Fergusson and Prof. Pritchard had appeared as witnesses against Vivisection at the Norwich prosecution, and the other gentlemen had more or less written against the practice, or had published leaflets and pamphlets against it. They all appeared before the Royal Commission except Mr. Goodson, Mr. Torr, Mr. Brewin, Dr. May, and the sixty medical signatories alluded to.

THE QUEEN'S LETTER.

Letter addressed by command of Her Most Gracious Majesty the Queen to the President of the Royal Society for the Prevention of Cruelty to Animals, by Lieutenant-General Sir T. M. Biddulph, K.C.B., referred to in p. xlii.

Buckingham Palace, 10th June, 1874.

MY DEAR LORD,

THE QUEEN has commanded me to address you as President of the Society for the Prevention of Cruelty to Animals, on the occasion of the assemblage in this country of the foreign delegates connected with similar associations, and of the jubilee of the Society, to request you to give expression publicly of Her Majesty's warm interest in the success of the efforts which are being made here and abroad for the purpose of diminishing the cruelties practised on dumb animals. The Queen hears and reads with horror of the sufferings which the brute creation often undergo from the thoughtlessness of the ignorant, and she fears also sometimes from experiments in the pursuit of science. For the removal of the former, the Queen trusts much to the progress of education; and in regard to the pursuit of science, she hopes that the entire advantage of those anæsthetic discoveries from which man has derived so much benefit himself, in the alleviation of suffering, may be fully extended to the lower animals. Her Majesty rejoices that the Society awakens the interest of the young by the presentation of prizes for essays connected with the subject, and hears with gratification that her son and daughter-in-law show their interest and sympathy by presenting those prizes at your meeting. Her Majesty desires me to announce a donation of one hundred guineas towards the funds of the Society.

I am, &c.,

T. M. BIDDULPH,

The Earl of HARROWBY, K.G., &c., &c.

Memorial addressed to the R.S.P.C.A. on the 25th day of January, 1876.

The practice of Vivisection has received of recent years enormous extension. Instead of an occasional experiment, made by a man of a high scientific attainment, to determine some important problem of physiology, or to test the feasibility of a new surgical operation, it has now become the every-day exercise of hundreds of physiologists and young students of physiology throughout Europe and America. In the latter country lecturers in most of the schools employ living animals instead of dead for ordinary illustrations; and in Italy, one physiologist alone has for some years past experimented on more than 800 dogs annually. A recent correspondent in *The Spectator* shows that many English physiologists contemplate the indefinite multiplication of such vivisections; some (as Dr. Pyc-Smith) defending them as illustrations of lectures, and some (as Mr. Ray Lancaster) frankly avowing that one experiment must lead to another *ad infinitum*. Every real or supposed discovery of one physiologist immediately causes the repetition of his experiments by scores of students. The most numerous and important of these researches being connected with the nervous system, the use of complete anaesthetics is practically prohibited. Even when employed during an operation, the effect of the anaesthetic shortly ceases, and, for the completion of the experiment, the animal is left to suffer the pain of the laceration to which it has been subjected. Another class of experiments consists in superinducing some special disease; such as alcoholism (tried by M. Magnan on dogs at Norwich), and the peculiar malady arising from eating diseased pork (Trichiniasis), superinduced on a number of rabbits in Germany by Dr. Virchow. How far public opinion is becoming deadened to these practices is proved by the frequent recurrence in the newspapers of paragraphs simply alluding to them as matters of scientific interest involving no moral question whatever. One such recently appeared in a highly respectable Review, detailing a French physiologist's efforts, first to drench the veins of dogs with alcohol, and then to produce spontaneous combustion. Such experiments as these, it is needless to remark, cannot be justified as endeavours to mitigate the sufferings of humanity, and are rather to be characterised as gratifications of the "dilettantism of discovery."

The recent trial at Norwich has established the fact that, in a public Medical Congress, and sanctioned by a majority of the members, an experiment was tried which has since been formally pronounced by two of the most eminent surgeons in the kingdom to have been "cruel and unnecessary." We have, therefore, too much reason to fear that in laboratories less exposed to the public view, and among inconsiderate young students, very much greater abuses take place, which call for repression.

It is earnestly urged by your Memorialists that the great and influential Royal Society for the Prevention of Cruelty to Animals may see fit to undertake the task (which appears strictly to fall within its province) of placing suitable restrictions on this rapidly increasing evil. The vast benefit to the cause of humanity which the Society has in the past half-century effected, would, in our humble estimation, remain altogether one-sided and incomplete, if, while brutal carter and ignorant costermonger are brought to punishment for maltreating the animals under their charge, learned and refined gentlemen should be left unquestioned to inflict far more exquisite pain upon still more sensitive creatures; as if the mere allegation of scientific purpose removed them above all legal or moral responsibility.

We therefore beg respectfully to urge on the Committee the immediate adoption of such measures as may approve themselves to their judgment as most suitable to promote the end in view, namely, the *Restriction of Vivisection*; and we trust that it may not be left to others, who possess neither the wealth or organisation of the Royal Society for the Prevention of Cruelty to Animals, to make such efforts in the same direction as might prove to be in their power. If the Committee do us the honour to ask our opinion as to how such a work can be carried on, we beg respectfully to indicate some of the ways which we have had in contemplation in drawing up this Memorial.

1st. By the appointment of a Sub-Committee expressly to deal with the subject, to be called the "Sub-Committee for the Restriction of Vivisection."

2nd. By instructing Mr. Colam to undertake as many prosecutions of cases of Vivisection involving severe animal suffering as may prove to come within the scope of the existing law, and so to bring the matter prominently before the public eye.

3rd. Should it be found that grounds for such prosecutions are rarely to be obtained, owing to the secrecy with which Vivisections are usually performed, or

that, when undertaken the state of the law renders them ineffectual.—it may then be considered whether a Bill should not be introduced into Parliament making the publication of any cruel experiment in a scientific journal or other work, a legal ground for the prosecution of the publisher, and thus throw upon the operator the *onus* of justifying the act. In view of the ambition for scientific notoriety, which may be deemed a not insignificant motive for the performance of many of these experiments, it is believed that this provision would be eminently effective.

4th. If a Bill on the subject were found advisable, it might properly contain other provisions, such as, 1st, the prohibition of all painful experiments on animals except in authorised laboratories and by registered persons, whose experiments should be also registered as to number, nature and purpose; 2nd, the absolute prohibition of painful experiments as illustrations of lectures; and 3rd, the extension to three months of the interval of a single month now allowed by law for the prosecution of cruelty to animals after the commission of the offence. All the provisions of such an Act would of course be carefully weighed by Parliament in debate; and while physiologists would contend for such liberty as they might be enabled to justify to the conscience of the nation, the Society would endeavour to obtain security against its abuse.

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The Primate of Ireland
The Archbishop of Dublin
The Archbishop of Westminster
H.S.H. Prince W. A. of Saxe-Weimar
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 bridge
 Edward Caird, Prof. Moral Philosophy,
 Glasgow
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 Prof. John Wilson, Edinburgh
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 Samuel Davidson, D.D., LL.D.
 Samuel Haughton, D.D., M.D., Dublin
 Prof. Blackie, Edinburgh
 Principal Shairp, St. Andrew's
 Rev. B. Jowett, Master of Balliol
 The Provost of Trinity College, Dublin
 Sir William Fergusson, Bart.
 Dr. Cotton (Senior Physician, Brompton
 Hospital)
 Mr. Bellamy, Charing Cross Hospital
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 C.S.G.
 J. Griffith Loch, L.R.C.P., M.R.C.S.
 Charles Mosse, C.B., Surgeon-Major

John Henry Sylvester, Surgeon-Major
 Charles Taylor, M.D.
 S. C. Hornsby Wright, M.D.
 Thomas Worth, M.R.C.S.,
 Joseph White, F.R.C.S.
 Charles West, M.D., Wimpole-street
 G. H. Porter, Surgeon to the Queen in
 Ireland
 W. Stokes, M.D., Dublin
 Jolliffe Tufnell, President, Royal Col-
 lege of Surgeons in Ireland
 B. G. M'Dowel, Professor of Anatomy
 James J. Duncan, M.D., President of
 the King's and Queen's College of
 Physicians, Ireland.

BILL OF THE R.S.P.C.A.

Whereas it is expedient to abolish the practice of painful Vivisections and the performance of other experimental operations upon animals, as well as to prohibit the instruction of students, in classes and otherwise, by such means. Be it enacted:

1. The term "Vivisection" in this act shall be taken to mean every experiment producing or of a nature to produce pain or disease in any animal, as well as the cutting or wounding of any living animal otherwise than for the purpose of curing or alleviating some disease with which such animal is affected, or of killing it; the term "anæsthetic" in this act shall be taken to mean any agent which will produce complete insensibility to pain.

2. It shall not be lawful after the first day of January, one thousand eight hundred and seventy-six, to perform a vivisection save in a place which is registered in pursuance of this Act.

Notice of any place in which it is intended to perform vivisections shall be given in Great Britain to one of Her Majesty's Principal Secretaries of State (in this Act referred to as the Secretary of State), and in Ireland to the Chief Secretary to the Lord Lieutenant of Ireland (in this Act referred to as the Chief Secretary); such notice shall be signed by a member of some college of physicians or surgeons, and shall sufficiently describe the place in respect of which it is given; and the Secretary of State or Chief Secretary may cause any place with respect to which such notice has been given, to be entered on a register to be kept for the purposes of the Act in such form and under the management of such persons as the Secretary of State and the Chief Secretary may respectively direct.

Subject to the consent of the Secretary of State and Chief Secretary respectively as aforesaid, an entry on the register in pursuance of this section may continue in force for twelve months from the date thereof and no longer; but such entry may be at any time renewed on a fresh notice being given as required by this section.

Any person who, after the first day of January one thousand eight hundred and seventy-six, performs a vivisection in a place which is not registered in conformity with this section, shall be deemed to have committed an offence against this Act.

3. From and after the passing of this Act no person shall perform, or cause to be performed, or take part in performing except as hereinafter provided any Vivisection without having previously obtained a license as hereinafter is provided.

4. That it shall be lawful for Her Majesty's Principal Secretary of State and for the Chief Secretary for Ireland as aforesaid, immediately on the passing of this Act, or so soon thereafter as may be required, to grant a license to practise Vivisection to any Fellow or Member of any College of Physicians or Surgeons, or to any Graduate or Licentiate of Medicine, or to any person lawfully qualified to practise medicine in any part of the United Kingdom, or to any Professor or Teacher of Anatomy, Medicine, Surgery, Physiology, on application from such person for such purpose in the form set forth in the Schedule to this Act accompanied by a certificate in the form set forth in the same Schedule, and signed by one at least of the following persons, viz., the President of the Royal Society, the Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin, the Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin, and also by a Professor of Physiology, Medicine, or Anatomy in some University in Great Britain, or recognized by the Colleges of Surgeons and Physicians aforesaid.

5. And whereas under and in pursuance of an Act passed in the second and third years of the reign of King William the Fourth intituled "An Act for regulating Schools of Anatomy," certain Inspectors of places, where Anatomy is carried on, are appointed for certain Districts in Great Britain and Ireland, be it enacted that, immediately upon the registration of a place, or the granting of a licence for the practice of Vivisection, the said Principal Secretary of State, or Chief Secretary as aforesaid respectively, shall give notice of such registration or licence having been granted to the Inspector so appointed for the district in which the applicant for such registration or license shall reside.

6. Every such license may at any time be revoked by such Secretary of State and Chief Secretary respectively.

7. No person shall perform or cause to be performed, or take part in performing any Vivisection while lecturing to, or giving instruction to Students in Classes or otherwise, or for the illustration of lectures in Schools, Hospitals, or Colleges, or in any other places.

8. No person shall perform or take part in performing any Vivisection for the purpose of attaining manual skill.

9. No person shall perform or cause to be performed, or take part in performing any Vivisection upon any animal without having first subjected such animal to the influence of an anæsthetic so as to render it wholly insensible to pain.

10. No person who shall perform, or cause to be performed, or take part in performing any Vivisection upon any animal so subjected as aforesaid, shall omit to destroy such animal before the effect of the anæsthetic ceases.

11. A license under this Act shall extend to any person assisting the holder of the license, provided the person assisting, acts in the presence and under the direction of such holder.

12. A Justice of the Peace, on information on oath that there is reasonable ground to believe that Vivisections are performed at any place not registered, or by any person not licensed in pursuance of this Act, may issue his warrant, authorising any officer of police to enter and search such place where it is alleged that such Vivisections are being carried on, and to take the names and addresses of the persons found therein.

Any person who refuses admission on demand to a police officer so authorised, or who obstructs such officer in the execution of his duty under this section, or who refuses on demand to disclose his name and address, or who gives a false name or address, shall be liable to a penalty not exceeding Five Pounds, or to a term of imprisonment not exceeding two months.

13. A book or books shall be kept by every Inspector of places where Anatomy is carried on of all licenses granted by Her Majesty's Principal Secretary of State, or by the Chief Secretary as hereinbefore mentioned, together with the names, surnames, descriptions and addresses of the persons to whom such registrations or licences have been granted, together with the date of the granting or revocation of all such registrations or licences, and such book or books shall be open at all reasonable times to inspection by any person on payment of two shillings.

14. Every person who shall be so licensed as hereinbefore is provided, shall keep a book at the place registered as hereinbefore provided, in which he shall enter in plain words and figures and with consecutive numbers the particulars of each and every Vivisection which he shall perform or take part in performing, the object and result of such Vivisection, the kind of animal upon which such Vivisection is performed, the kind of anæsthetic used, and the period of time which elapsed between the time at which the anæsthetic first took effect and the destruction of the animal, together with the name, surname, description and address of the person from whom such animal was received. Such book shall be in the form set forth in the Schedule to this Act annexed, and such book, and the place registered as hereinbefore provided shall at all times be open to the inspection of such Inspector of Anatomy so appointed as aforesaid, or of Her Majesty's Principal Secretary of State or such Chief Secretary as aforesaid, or of such person as they respectively may appoint for the purpose.

15. Any person offending against any of the provisions of this Act shall forfeit and pay a penalty not exceeding Ten Pounds for the first offence, or imprisonment for a term not exceeding three months, and a penalty not exceeding Fifty Pounds for the second and every subsequent offence, or imprisonment for a term not exceeding six months, upon summary conviction before a Justice of the Peace.

[Then follow the usual sections relating to jurisdiction in Ireland and Scotland, and to appeals, Schedule, etc., etc.]

A Bill intituled An Act for regulating the Practice of Vivisection. (Presented by the Lord Hartismere ; ordered to be printed 4th May, 1875.)

1. This Act may be cited as the Vivisection Act, 1875.

2. It shall not be lawful, after the first day of January one thousand eight hundred and seventy-six, to perform a vivisection save in a place which is registered in pursuance of this Act.

Notice of any place in which it is intended to perform vivisections shall be given in Great Britain to one of Her Majesty's Principal Secretaries of State (in this Act referred to as the Secretary of State), and in Ireland to the Chief Secretary to the Lord Lieutenant of Ireland (in this Act referred to as the Chief Secretary); such notice shall be signed by a member of some college of physicians or surgeons, and shall sufficiently describe the place in respect of which it is given; and the Secretary of State or Chief Secretary shall cause any place with respect to which such notice has been given, to be entered on a register to be kept for the purposes of this Act in such form and under the management of such persons as the Secretary of State and Chief Secretary may respectively direct.

An entry on the register in pursuance of this section shall continue in force for twelve months from the date thereof and no longer; but such entry may be at any time renewed on a fresh notice being given as required by this section.

Any person who, after the first day of January one thousand eight hundred and seventy-six, performs a vivisection in a place which is not registered in conformity with this section, shall be deemed to have committed an offence against this Act.

3. Any inspector of anatomy may at any time visit and inspect any place which is for the time being registered in pursuance of this Act.

4. A vivisection shall not (save as herein-after mentioned), after the first day of January one thousand eight hundred and seventy-six, be performed on any animal until such animal has been placed completely under the influence of an anæsthetic; and for the purposes of this section the substance called urari or curare shall not be deemed to be an anæsthetic.

Any person may apply to the Secretary of State or Chief Secretary for a special license to perform vivisections without the use of anæsthetics, and the Secretary of State or Chief Secretary may, as he thinks fit, grant or refuse such license.

There shall be paid in respect of every such license the sum of ten pounds, and every such license shall continue in force for six months, and no longer.

Any person who performs a vivisection in contravention of this section shall be deemed to have committed an offence against this Act.

5. The Secretary of State or Chief Secretary may remove any registered place from the register on its being proved to his satisfaction that any provision of this Act has been contravened in such place.

6. A justice of the peace, on information on oath that there is reasonable ground to believe that vivisections are performed at any place not registered in pursuance of this Act, may issue his warrant authorising any officer of police to enter and search such place, and to take the names and addresses of the persons found therein.

Any person who refuses admission on demand to a police officer so authorised, or who obstructs such officer in the execution of his duty under this section, or who refuses on demand to disclose his name and address, or who gives a false name or address, shall be liable to a penalty not exceeding five pounds.

7. Any person who commits any offence against this Act for which no other penalty is imposed shall be liable to a penalty not exceeding twenty pounds.

All offences and penalties under this Act may be prosecuted and recovered in manner directed by the Summary Jurisdiction Acts before a court of summary jurisdiction. [Then follow jurisdiction clauses.]

A Bill to prevent Abuse in Experiments on Animals made for the purpose of Scientific Discovery. (Prepared and brought in by Mr. Lyon Playfair, Mr. Spencer Walpole, and Mr. Evelyn Ashley ; ordered to be printed 12th May, 1875.)

Whereas it is expedient to prevent cruelty and abuse in the experiments made on living animals for the purpose of promoting discoveries in the sciences of medicine, surgery, anatomy, and physiology: Be it enacted, etc.

1. Save as herein-after mentioned, no person shall, for any purpose whatever, make an experiment causing pain, or of a nature to cause pain, on any live animal.

Any person acting in contravention of this section shall be guilty of a misdemeanor, or shall be liable, on prosecution before a court of summary jurisdiction, to a penalty not exceeding *fifty pounds*, or imprisonment for a term not exceeding *three months*.

2. Any person, for the purpose of new scientific discovery, but for no other purpose, shall be permitted to make an experiment on a live animal of a nature to cause pain notwithstanding this Act and the Prevention of Cruelty to Animals Act (passed in the twelfth and thirteenth years of Her present Majesty, chapter ninety-two), provided that the following conditions are complied with:

- (1.) That the animal shall first have been made insensible by the administration of anæsthetics or otherwise, and shall continue to be insensible during the whole of such experiment; and
- (2.) That, if the nature of the experiment be such as to seriously injure the animal, so as to cause to it after-suffering, the animal shall be killed immediately on the termination of the experiment.

3. Nothing in this or in the Prevention of Cruelty to Animals Act contained shall be taken to prohibit any person holding a license, as herein-after provided under this Act, from subjecting any live animal to an experiment without the use of anæsthetics, provided that in making such experiment the following conditions are all complied with:—

- (1.) That the experiment is made for the purpose of new scientific discovery, and for no other purpose; and
- (2.) That insensibility cannot be produced without necessarily frustrating the object of the experiment; and that the animal should not be subjected to any pain which is not necessary for the purpose of the experiment; and
- (3.) That the experiment be brought to an end as soon as practicable; and
- (4.) That if the nature of the experiment be such as to seriously injure the animal so as to cause to it after-suffering, the animal shall be killed immediately on the termination of the experiment;
- (5.) That a Register of all experiments made without the use of anæsthetics shall be duly kept, and be returned in such form and at such times as one of Her Majesty's Principal Secretaries of State may direct.

4. Any person desirous to obtain a license under this Act may apply for the same to one of Her Majesty's Principal Secretaries of State. Such application shall be in the form set forth in the Schedule to this Act, and shall be signed by the applicant, and shall be accompanied by a certificate in form set forth in the same Schedule, and signed by one at least of the following persons; viz., the President of the Royal Society, the Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin, the Presidents of the Colleges of Physicians in London, Edinburgh, or Dublin, and also by a professor of physiology, medicine, or anatomy in some university in Great Britain, or recognised by the Colleges of Surgeons and Physicians aforesaid.

Provided that if the applicant be a professor or lecturer of or in physiology, medicine, anatomy, or surgery in any university in Great Britain, or in any college incorporated by Royal Charter, or a professor or lecturer in any one of such sciences employed by Government, such a certificate shall not be required; but, instead thereof, his application shall be countersigned by the registrar, president, principal, or secretary of such university or college.

5. Upon receiving such application the Secretary of State may, if he think fit, grant to the applicant a license under this Act.

6. A license under this Act shall, unless earlier revoked, be in force, in the case of the holder being a professor or lecturer as herein-before mentioned, during such time as he shall continue to hold such professorship or lectureship, and in any other case for the term of five years. Provided that from time to time, when a license expires a Secretary of State shall renew the same upon receiving a written application from the holder, without requiring a fresh certificate or any countersigning of the application.

7. A Secretary of State may at any time, on cause shown, revoke any license granted under this Act.

8. A license under this Act shall extend to any person assisting the holder of the license, provided the person assisting acts in the presence and under the the directions of such holder.

9. All offences and penalties under this Act may be prosecuted and recovered in manner directed by the Summary Jurisdiction Acts before a court of summary jurisdiction. [Then follow Jurisdiction and Appeal Clauses.]

DOCUMENTARY EVIDENCE.

P R E F A C E.

It has been thought best to introduce this evidence by a description of apparatus required for operations, written by Dr. Brunton, which corresponds with similar directions in the "Handbook for the Physiological Laboratory," edited by Dr. Burdon-Sanderson, and other handbooks.

References are added to all extracts.

If it be alleged that, although the authors do not state it, the animals were nevertheless narcotised in some of the experiments, it may be said in reply that no rule exists for the production of anæsthesia in laboratories except the operator's conscience and his convenience in manipulation, and that vivisectors sometimes are of opinion that the presence of pain is preferable to the presence of a narcotic. It may also be added that when animals have to be narcotised the writers generally state such circumstance, and the omission therefore adds weight to the foregoing remarks. Even when used a narcotic may be insufficient in quantity or ineffectual in quality to prevent pain, although effective to permit of easier experimentation. The object may be moreover to deaden the sensory nerves during the cutting process only, and in such cases the animal awakens from its enforced sleep to the terrible consciousness of torture. Curare does not destroy pain, but only the power to express it. All these points must be remembered in reading the evidence.

It is not expected that every one of the following quotations will pass without question; but it is assumed that the entire collection will present conclusive evidence that pain is very often inflicted by Vivisectors, much of which is prolonged.

The extracts are made from English books, and where the experiment was performed on the Continent it will be found that an English sanction has been given to it.

Four divisions have been made, viz. :—(*a.*) Pain. (*b.*) Prolonged Pain. (*c.*) Design to teach Students Vivisection. (*d.*) Opinions more or less against Vivisection.

(a.)

EVIDENCE OF PAIN.

(1.)—*Mode of securing Animals.*—In order to determine in an exact manner what organs or parts are affected, we are obliged to make use of apparatus of various kinds; and before these can be applied to an animal it must be prevented from moving. Frogs are fastened to a frog-board by a piece of cord with a noose at the end, slipped over each elbow and ankle. The frog board may consist of a piece of millboard about nine inches long by three inches broad, with four slits at the sides to keep the cords in position, or of a piece of wood the same size, and from a quarter to half an inch thick, with holes, through which the cords are passed. They may be fastened by simply tying them together, or by sticking a small wooden pin into each hole, or by four screws, such as are used for fastening the wires of galvanic batteries, placed in the edges of the board. The last way is, I think, the most convenient. Rabbits are best secured by Czermak's holder and board.

[*Here follows drawing of Czermak's holder, with the following key .—*]

{Czermak's Rabbit-holder and Board.—A, the board. B, a bent piece of iron forming the upper part of the board. C, an open space through which instruments can be introduced from below to divide the spinal cord. It is generally covered by an iron plate. D is an upright rod fixed by a screw into a slit in B. F is a forked rod, which can be moved back or forward, up or down, by the nut E. The forks are hollow, so that the ends of the holder can be passed into them and fastened by the screw *j*. H is a bar which passes behind the incisor teeth of the rabbit. G and G are two bent bars which pass under the chin and over the nose of the animal, and are brought together by the screw *l*. From the upper end of *g'* hangs a screw, which passes between two projections on *g*, and has a mother-screw, K. The screw K works against the projections of *g*, and draws the ends of *g'* and *g* together. These press on the rabbit's nose and under jaw, and keep the teeth firmly locked over the rod *h*. M M are screws for fixing the cords which confine the legs. They

are a remarkably convenient sort, consisting of an outer part with a horizontal hole, and an inner ring with a stalk, on which a milled screw plays when the milled head is at the top of the stalk; the inner ring and outer holes correspond, and the cord can be easily pushed through; but when the milled head is turned, the stalk and ring are drawn up and the cord nipped between it and the outer part. The cords may either be fastened directly in the screw, or passed first through one of the holes in the edge of the board. The board should be covered with a large pad of india-rubber stuffed with horse-hair, and there should be another round pillow to put under the animal's neck.)

The best cord is strong window cord. The one end should be flattened with a hammer, and turned over so as to make a small loop, whose two sides are then firmly bound together with waxed thread. Through this loop the other end is passed, and the noose thus made is ready to be drawn tight at any moment. The other end of the cord should be cut to a point, and also bound with waxed thread, to prevent the strands unravelling. The rabbit is placed on the board, the nooses slipped over the legs and drawn tight, and the ends of each cord passed through the screw which will be nearest it when the animal lies on its back. The rabbit is then turned over, and the cords drawn through the screws and fastened. The bar *h* is then put between its teeth, and the screw *l* turned till *g* and *g'* fit tightly over its muscle, and the projecting ends of *g* fixed into the ends of *f*. Dogs may be fastened by Bernard's holder.

[Here follows drawing of Bernard's holder, with the following key:—]

(A is Bernard's dog holder—*a* is a metal ring, within which a bent piece of metal, *b*, is moved up and down by the screw *c*. *h* is a straight piece, which is fastened by a screw to *a*, and can be moved nearer to or farther from a corresponding piece at *b*. These two pieces lie under the lower jaw of the dog; the bent piece *b*, is screwed down on its nose, and the strap *i*, buckled behind its head, which is thus firmly fixed. It may be moved back or forward by sliding the rod *d* through the nut *e*, or up and down by moving *e* on *f*, which is a strong iron rod fastened to a table or board by the screw *g*.)

(B, Brunton's holder for dogs or rabbits.—A loop of cord is tied round the upper jaw, the bar *l* passed behind the canine teeth of the dog or cat or incisors of the rabbit, and the two jaws then tied together to prevent its slipping out. This mode of fastening animals has been long used, and my modification simply consists in the addition of the forked bar *k*. After *l* is fastened in the mouth, the forked ends of *k* are pushed through holes in *l*, and fastened by the screws *m*. *k* may then be fastened to an upright bar by means of a nut in the same way as Bernard's or Czermak's holder.) or by a simple bar of iron put behind their canine teeth. A piece of cord is first tied round the upper jaw, the bar put into the mouth, and

the two jaws tied firmly over it. A split strap may be used instead of the cord. I have had a bar made with a hole at each end, into which a fork of steel passes and is secured by a screw. The fork may then be fastened by a nut to an upright rod, as in Czermak's holder (Fig. 2, B). Cats and guinea-pigs may be fastened by Czermak's holder. For guinea-pigs a little padding must be placed between g and g' , in order to make them catch the head. A simple bar and cord may also be used for rabbits, cats, and guinea-pigs, as well as for dogs.

Narcotising Animals.—Narcotics cannot be given in all cases to animals on which we experiment, as their action must to a certain extent complicate that of the drug which we wish to investigate. We cannot use them when we are observing what are the general symptoms which a medicine produces. But when we are investigating its action on particular organs we may often use them, not only with safety but with advantage, when they have no action on the particular organ which we are studying, or so little that its disturbing influence is more than compensated by the diminished muscular action and consequent ease in performing the experiment which narcotics produce. It is almost unnecessary to say that, in all cases which admit of it, narcotics should be used, as we have no right to inflict any unnecessary pain, although we may be justified in taking the lives of the lower animals in order to preserve the more valuable life of man, either by supplying him with food by means of those killed in the slaughterhouse, or by obtaining the knowledge which shall enable us to cure disease by means of those killed in our experiments. The narcotics which we use are opium and chloral. Chloroform is inadmissible, as its administration generally seems to cause dogs more pain than the experiment itself, and rabbits are very easily killed by it.

When we wish to render the animal absolutely motionless, or to observe what effect any drug will produce after the motor nerves have been paralysed, we give curare.

Curare may be obtained from Messrs Hopkin and Williams, New Cavendish Street, London, or from Bruckner and Lampe.—*Dr. Brunton, British Medical Journal*, No. 542, pp. 321-2.

(2.)—An experiment, illustrated by a drawing, shows a living frog strapped down to a board, its sciatic nerve dissected out, bared, and brought out of its body from its thigh to its loins, and attached to a galvanometer, after which strychnia is introduced under its skin, and a result from the action of the tortured nerve is shown on the needle of the instrument.—*Dr. Radcliffe, Lancet*, No. 2,061, p. 227.

(3.)—If the sciatic nerve of a rabbit be divided in the ham, and

the end which is in connection with the brain be dissected out and laid across the poles of a galvanic cell, the animal screams with pain and strains with convulsive movements when the circuit is closed or opened. Before the time when the portion of the nerve which is included in the circuit is paralysed by the current, the screams and convulsions happen equally at the closing and opening of the circuit, and it is immaterial whether the positive or the negative pole be in the position next the brain. After the time when the portion of nerve which is included in the circuit is paralysed by the current, the screams and convulsions are present at the closing of the circuit and absent at the opening when the negative pole is in position next to the brain, and absent at the closing of the circuit and present at the opening when the positive pole is in the position next the brain. Pain and convulsion, that is to say, come together and go together. In a word, there is reason to believe that the electrical changes which a sentient nerve experiences in the production of sensation are the exact equivalents of the electrical changes which a motor nerve experiences in the production of muscular contraction.—*Dr. Radcliffe, Lancet*, No. 2,067, p. 409.

(4.)—Through a fistulous opening into the stomach of a dog Bernard introduced, whilst digestion was going on, the hind legs of a living frog. The legs were dissolved away, the animal continuing all the while alive, and living for some time even after the experiment was completed. I have repeated this experiment myself and obtained a similar result. It proves unquestionably that the stomach has the power of dissolving living substances.

I performed an experiment substituting the ear of a rabbit for the hind legs of a frog. Whilst my dog with a fistulous opening in its stomach was at a period of full digestion, I carefully introduced through the cannula the ear of a vigorous rabbit, and held it in position with the hands so as to avoid mechanically injuring it or producing congestion by obstructing the flow of blood through its vessels. At the end of two hours the ear was withdrawn, and several spots of erosion, some as large as a sixpenny-piece, were observed on its surface, but nowhere was it eaten completely through. On being replaced for another two hours and a half the tip, to the extent of about a half or three-quarters of an inch, was almost completely removed, a small remnant of it only being left attached by a narrow shred to the remainder of the ear. The gastric juice seemed to act like a strongly corrosive material, making first a number of ulcerated-looking spots through the skin, and afterwards extending its action more rapidly through the central parts of the ear. A rather profuse hæmorrhage took place, especially towards the latter part of the experiment. My own fingers became moistened with gastric juice that escaped by the side of the ear, and afterwards felt sore or tender

as if the skin had been slightly acted on.—*Dr. Pavy, Lancet*, No. 2,070, p. 492.

(5.)—The Parisian Correspondent describes his attendance at a meeting of the Academy of Sciences, and says:—These phenomena occur even when a drop of ammonia is inserted into the eye of an animal, and whilst the organ is strongly closed during the paroxysm of pain which ensues.—*Lancet*, No. 2,037, p. 298.

(6.)—Experiments were made on small animals. The spinal cord was artificially inflamed by having a thread passed through it. The appearances found were contraction of the axis-cylinders, subsequent fissiparous division of the contracted portions, and the formation of pus from these. The nerve cells were affected with granular or “œdematous” degeneration. The neuroglia was somewhat increased, but not to a great extent. The perivascular lymphatic spaces of the surrounding pia mater were filled with lymph-corpuscles. One or two cases were quoted in confirmation of the facts.—*The Doctor*, October 1st, 1875, p. 195.

(7.)—Mr. Bert has been instituting a series of experiments in illustration of “the phenomena and causes of the death of fresh-water animals when plunged into sea water.”

A frog when immersed in sea water is much agitated, and exhibits signs of pain, unless he can keep his muzzle above the surface. When all signs of sensibility have disappeared, the nerves and muscles are still found to be excitable, and the heart, filled with dark blood, is still seen to beat spontaneously.

The animal is found to have lost from one-fifth to one-third of its weight, the loss being chiefly borne by the muscles, which present a continuous or durable contraction like a kind of cramp.

Frog lived one hour in sea water.—*Edinburgh Medical Journal*, 1871-2, p. 473.

(8.)—By a new method of operation, a description of which is given in the original, they were enabled to remove the first thoracic ganglion without injury to the pleura, and in consequence, never observed pleuritis, as is common in the other methods of operation, and as said by some to depend upon the extirpation of the said ganglion.

On the contrary, neither increase of the temperature of the ear, nor of the fore foot, or the opposite side operated on were missed, also the oculo-pupillary phenomena will be the vascularisation of the conjunctiva, as occurs after section of the sympathetic in the neck.

Rotatory movements were absent.—*Journal of Anatomy and Physiology*, November, 1874, p. 213.

(9.)—For the production of coughing the author employed mechanical stimulants, feathers, pinching, teasing, squeezing with forceps, chemical irritants (common salt and ammonia), thermal (ice) and electrical stimuli were employed. [For these experiments on cats and dogs it is said the animals “in no case” were narcotised.]—*Journal of Anatomy and Physiology*, November, 1864, p. 218.

(10.)—Blindness of the opposite eye and paralytic dilatation of the corresponding pupil can be produced, while stimulation of the same spot is followed by strong and continued contraction of the pupil.—*Journal of Anatomy and Physiology*, February, 1875, p. 210.

(11.)—Having found in a large number of experiments on the effects of various agents on the temperature of the body and the generation of heat, where the effects of muscular exertion had been set aside by the injection of woorara, there was a constant and nearly equable fall of temperature, beginning from the period of narcosis after antecedent convulsive movements, he determined to follow out by careful experiment the action of the woorara alone.

Another series of experiments were made on animals (principally dogs), in which fever was excited by the injection of pus into the veins, and it was found that, even in this state, the temperature was lowered by the action of woorara to the normal average, or even below it.—*Lancet*, No. 2,510, p. 516.

(12.)—Nothnagel states that he found the method of injecting chromic acid, which he originally applied in order to determine the function of the convolitional centres, inappropriate for experiments on these deeper-seated centres; and he has employed, instead, a little cannula and trocar, from the extremity of which, by pressure on a lever in the handle, small diverging arms could be made to protrude, and by rotation the thalamus could readily be entirely broken down. The powers of motion and sensation in the animal were then carefully tested, whilst an examination after death revealed the precise nature of the injury inflicted. The trocar was always introduced at the side of the head, and passed through the cornu ammonis and a part of the hemispheres.

He states, as his final results, that destruction of the optic thalami causes neither motor paralysis nor cutaneous anæsthesia, but that his experiments support the view of Meynert, namely, that motor processes take place in the optic thalami, which are caused or excited by sensory impressions.—*Lancet*, No. 2,682, p. 135.

(r3.)—His experiments were conducted on dogs and rabbits fully under the influence of curare. A bent glass tube was inserted into the ductus communis choledochus in a large curarised rabbit, in which artificial respiration was maintained. He obtained at the commencement of an experiment lasting half-an-hour one drop of bile in every seventeen seconds.

Compression of the vena portæ, or of the hepatic artery, or of both together, caused an immediate diminution of the quantity of the secretion; but after some time, though the compression was continued, the liver recovered its activity to some extent. A rabbit which yielded one drop of bile in about twenty-four seconds, before compression, only yielded one drop of bile per seventy seconds after; and a dog, previously giving one drop in thirty seconds, only gave one per eighty or ninety seconds after compression. Ligature of the thoracic aorta, just above the diaphragm, reduced the secretion in a dog from one drop in seven seconds to one in fifty. Ligature below the cœliac axis raised the rapidity of the secretions from one drop in eight seconds to one drop in three seconds in a rabbit.—*Lancet*, No. 2,602, p. 56.

(14).—In the section on the Formation of Sugar in the Liver, Mr. Dalton gives the results of his experiments performed in 1869. He then found that if a portion of the liver of a dog was excised, whilst the animal is living, and immediately cut into thin pieces and thrown into boiling water . . . —*Lancet*, No. 2,532, p. 324.

(15.)—This view has been supported by two experiments intended to show :—(1) that bile pigment can be produced artificially from the bile acids by the action of concentrated sulphuric acid ; and (2) that colourless biliary acids, when injected into the veins of dogs, are converted in the blood of these animals into bile pigment. These experimental results are still the subject of much discussion. They have been controverted by Kuhne, Hoppe, G. Harley, &c., but confirmed by Staedeler, Neukaunn, Folwarczny, Röhrig, &c.—*Dr. Murchison, Lancet*, No. 2,642, p. 537.

(16).—The animals were the dog and the frog; and the mode of experimentation was that the mean rapidity of the blood in the carotid and crural arteries and in the crural vein was ascertained; the sciatic or crural nerve was then dissected out and divided where it leaves the pelvis, yet so that both extremities could be easily subjected to the action of electric currents. The animals experimented on were either narcotised by the injection of morphia, or poisoned with woorara till the voluntary muscles were completely paralysed, artificial respiration being maintained in the latter case. It was not forgotten that, under the full toxic influence of woorara, paralysis of

the vaso-motor nerves takes place.—*Lancet*, No. 2,528, p. 192.
[Observe, curare is not an anæsthetic.]

(17.)—By the investigations of Bernard and Brown-Séquard it has been established that the contractile elements of the blood vessels are presided over by the motor nerves. The largest vaso-motor nerve in the body is the great splanchnic nerve. It presides over the vessels of most of the abdominal viscera. It is sometimes necessary in physiological experiments to paralyse the blood vessels of the body generally. This is readily done by dividing the cervical portion of the spinal cord at some little distance below the medulla oblongata. If, after the section, we desire to stimulate the vaso-motor nerves generally, we require to place two electrodes in the cervical portion of the cord, one on a level with, say, the third, the other on a level with the fifth cervical vertebra. The contraction of vessels produced in this way may be readily seen in the frog. I have injected $\frac{1}{100}$ gr. of curare under the skin to paralyse the voluntary motor nerves, and so get rid of spasm of the voluntary muscles when we stimulate the spinal cord. I stretch the web, tie threads round two neighbouring toes, and fix them in slits on either side of a triangular window in a piece of cardboard. The back is uppermost. Two common sewing needles fixed in a piece of gutta serena are pushed between the scapulæ into the spinal cord.

In performing this experiment it is necessary to beware of giving too large a dose of curare. A powerful dose paralyses not only the voluntary motor but also the vaso-motor nerves. Do not give more than $\frac{1}{100}$ gr.—*Dr. Rutherford, Lancet*, No. 2,525, p. 69.

(18.)—If intense myosis be effected by the instillation of calabar bean into one eye, irritation of the central extremity of the divided sympathetic in the neck produces no effect, for the sympathetic is paralysed by the bean; but if a current of electricity be passed across the eye, the electrodes being placed on opposite sides of the cornea, immediate dilatation can be induced, the muscular fibres being then directly called into action. A similar proof of the existence of a dilatator was formerly furnished by Hirschmann after the pupil had been made to contract with nicotine.—*Lancet*, No. 2,421, p. 127.

(19.)—Dr. B. W. Richardson read a paper "On some effects of extreme cold on nervous action." This was a continuation of his experiments in relation to the freezing the centres of the nervous system. He explained that frozen frogs do not respire during insensibility.

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Freezing the brain necessarily produced a gradual slowness of the circulation, and even entire cessation of the heart's action.

Lastly, he referred to the effects of freezing and rapidly thawing the skin of certain regions of the body, especially in birds, which in them caused extreme irregularity of movement, and other signs of nervous disturbance.—*Lancet*, No. 2,348, p. 292.

(20.)—M. Cohnheim's first experiments were performed on the corneæ of frogs. Inflammation was excited by touching them in the centre with a point of nitrate of silver, the contact being maintained sufficiently long to destroy the epithelium completely, and to affect the corneal tissue.

On repeating these experiments on the rabbit similar results were obtained. His next experiments were made on vascular parts, and he chose the mesentery of frogs poisoned with woorara. Sufficient inflammation was excited by merely exposing the membrane to the air, which produced first hyperæmia, and after the lapse of twenty-four hours the exudation of a thin dull-grey gummy layer or fibrinous pseudo-membrane.—*Lancet*, No. 2,329, p. 506.

(21.)—Threads are passed behind the common innominate and left subclavian arteries of a rabbit, and arrangements made by which these threads may be tied and untied in a moment. On tying the ligatures the animal was violently convulsed, on untying the ligatures, about sixty seconds later, the convulsions, which were then raging at their height, were instantly brought to an end.

In this experiment the subclavian arteries of a rabbit are tied at their origin, and a ligature is also placed around the arch of the aorta, a little beyond the opening of the left subclavian artery. The ligatures, that is to say, are so placed as to bring about a result which is the very opposite of that which was secured in the last experiment. In that case the blood was cut off from the head and neck, and the circulation confined to the trunk and limbs; in this case the blood is cut off from the body and limbs, and the circulation confined to the head. In this case, that is to say, the vessels of the brain, medulla oblongata, upper part of the spinal cord, and the cervical ganglia of the sympathetic nerve, receive more than their proper share of the blood—for all the blood of the body is diverted in this direction—while the rest of the spinal cord and the thoracic and abdominal ganglia of the sympathetic nerve receive no blood at all. And what is the result? The result is paralysis of the parts behind the ligatures without convulsion. In one or two instances the paralysis was preceded by trembling, but in no instance was there convulsion or anything approaching to convulsion. It was ascertained also that this

absence of convulsion was not due to paralysis of the spinal cord from want of blood, for on compressing the carotids so as to prevent the flow of blood through these vessels, the animal was instantly seized with violent convulsion.—*Dr. Radcliffe, Lancet*, No. 1,908, p. 288.

(22.)—Solution of an alkaline citrate (from one to ten per cent.) was injected under the skin of the back or thighs of frogs. One of the sciatic nerves was then cut through ; and tetanus was induced by means of strychnia. After this had lasted from one to six hours, the thighs were separately broken up into small pieces and rubbed up with water.—*Dr. Gscheidlen, British Medical Journal*, No. 682, p. 118.

(23.)—Dr. Lauder Brunton has made some very interesting experiments as to the action of chloral. He finds that after large doses the temperature falls till it can no longer be measured by ordinary clinical thermometers. Having found out what dose will kill an animal when exposed to the air, he gave this quantity to two similar animals, wrapping one up in cotton wool. The one so wrapped up survived, while the other died. Then he found out the dose which was lethal after wrapping up in wool, and gave that to two similar animals, wrapping one up in wool, and putting the other in a warm chamber. The one in wool died, the other recovered. A larger dose still was fatal to the one in the warm chamber. These experiments throw much light upon the action of chloral hydrate.—*Dr. Fothergill, British Medical Journal*, No. 680, p. 46.

(24.)—A young man, who wore red socks, having been attacked by a very acute and painful vesicular eruption on both feet, M. Tardieu attributed his affection to the red colour of the coralline dye. The substance in question having been separated by M. Roussin, the chemist, and injected under the skin of a dog, a rabbit, and a frog, which died from its effects, it was concluded to be a violent poison, and subsequently fell out of use as a dyeing agent. Contradictions of this statement, however, were soon forthcoming. M. Laudrin, a veterinary surgeon, asserted that he had administered coralline to dogs and cats without observing any subsequent ill-effect. He had had positive proof of the absorption of the coralline and of its purity, since he had been able to collect it in the lungs of the animals, and to dye silk with it. Dr. Guyot confirmed these experiments, and came to the conclusion that coralline was not poisonous, even in large doses, and that it may safely be used in dyeing, provided that it be not mixed with poisonous substances.—*British Medical Journal*, No. 719, pp. 467-8.

(25.)—Hieberg finds that when the epithelium is scraped off the surface of the cornea in the frog, fowl, or rat, it is reproduced only at the edge of the denuded part, and never from any isolated centre on its surface.—*Journal of Anatomy and Physiology*, 1871-2, p. 247.

(26.)—Bernard investigates the action of temperature by placing birds and rabbits in cages heated by a lamp or surrounded by a double metallic case containing a hot solution of sulphate of soda.

When the animals are thus exposed to a dry heat of 150° Fahr. the respirations become quick and tremulous, the temperature of the body rises, the heart beats quickly, and after a little time stops suddenly if the temperature be high enough. The necessary temperature is sooner reached in birds than in rabbits. It rose in birds to 122°, in rabbits to 115° Fahr. Rigor mortis came on quickly, and both arteries and veins contained black blood.—*Journal of Anatomy and Physiology*, 1871-2, p. 236.

(27.)—A few days ago I had the pleasure of assisting at several experiments of the highest interest, which Dr Brown-Séquard conducted at his laboratory of the Ecole Pratique.

The experiments which M. Bernard has performed on the circulation of the blood are of great interest. Amongst other facts and inferences the following may be briefly related. Circulation is accelerated or slackened according as such-and-such a nerve is operated upon. In one of the experiments the following facts were observed. A dog was submitted to the action of woorara. When quite motionless, respiration and circulation continuing, the extremity of one ear was cut off, and the quickness with which the drops of blood fell one after another served to note the speed of circulation. A glass tube was then introduced into the submaxillary gland so as to gather the secretion. The great sympathetic was then set bare in the neck, as also the sciatic nerve in the posterior region of the thigh. The cervical string of the sympathetic was then cut, and then the blood was seen to flow much more rapidly, the drops quickly succeeding each other. At the same time a flow of saliva from the submaxillary gland into the tube was observed. Just then, however, on galvanising the upper end of the great sympathetic the blood diminished and finally ceased running, through the contraction of the small arteries of the ear, the contraction sometimes going so far as completely to obliterate the arteries. These phenomena changed as galvanisation of the sympathetic was suspended.—*Lancet*, No. 2,550, p. 63.

(28.)—Their experiments were performed upon, for the most part, twenty curarised animals.

At the commencement they found not unfrequently that a stimulus which in one animal excited the most violent uterine contractions, in

another was apparently wholly inoperative, and this not only in animal of different genera, but in those of the same species. In rabbits, for instance, it sometimes happened that the slightest mechanical stimulus called forth active contractions, whilst in another animal no response occurred to the strongest electrical currents. They find that the animals best adapted to exhibit the movements are young but sexually mature rabbits, which have not yet been impregnated. In these the uterus, when the abdominal cavity is laid open, appears as a flat bandlike pink-coloured organ, at perfect rest, and but rarely exhibiting spontaneous movements. Their first experiments were made to determine the effect of arrest of the respiration. Krause, Mayer, and Basch have all shown that asphyxia induces movements of the intestines; but it has not hitherto been shown that a similar effect is produced upon the uterus. Oser and Schlesinger's experiments, however, show that in from ten to thirty seconds after suspension of the respiration contractions are perceptible, commencing from the tubæ and cervix, which in a few seconds more, extend over the whole uterus. This organ becomes pale, cylindrical, and rigid, and moves downwards and towards the middle line; the cornua raised themselves in an arched manner, and, intercoiling almost form a ball. The contractions increase in vigour with continued arrest of the respiration for some minutes. In a second set of experiments the effects of compression of the aorta were investigated, and it was ascertained that general contractions of the uterus occurred in a period varying from eighty to a hundred and twenty seconds after.

Küssmaul and Jenner long ago described a somewhat similar experiment, but gave a different explanation of the phenomena observed. In a pregnant rabbit they exposed and compressed the carotids and vertebrae. Extrusions of the foetuses occurred, but it was accompanied by convulsions, but they attributed the delivery to these rather than to any contractions of the uterus. MM. Oser and Schlesinger further found that, after section of the spinal cord in the cervical region, the contractions of the uterus no longer occurred more rapidly after arrest of the respiration than after compression of the aorta, in each case, supervening in about the same space of time, that is to say, in about a hundred seconds. After section of the cord neither general loss of blood, nor arrest of the flow of arterial blood to the brain produced any perceptible contractions.

The authors of the paper do not attempt to fix the situation of the centre for the uterine movements, but they point out that their experiments prove that it lies at a higher point than that at which they divided the spinal cord, viz.: between the occiput and atlas; and they think it is probable that it occupies some part of the medulla oblongata. At all events it is unlikely that it is situated in

the spinal cord, since the experiments of Küssmaul and Jenner and Schiffer show very clearly that the circulation of dyspnoëic blood (a term that implies no theory as to whether the phenomena produced are due to the absence of oxygen or the presence of carbonic acid) through the cord is not followed by any symptoms of irritation, as by convulsions, but by rapid paralysis of the posterior extremities, the paralysis appearing in the course of a minute or of a minute and a half.—*Lancet*, No. 2,551, p. 87.

(29.)—Professor Cyon gives the following results of experiments made upon the above points. Dogs and rabbits were used, some under the influence of curare during the experiments, and some not.

1. The uterine plexus is the most important, if not the only motor nerve which can produce effectual movements of the uterus by the irritation of its peripheral ends. Irritation of the central ends only gave rise to severe vomiting.
2. Irritation of the central ends of the first two sacral nerves produces in a reflex way powerful uterine movements which cease after the uterine plexus has been cut through. Irritation of the peripheral nerves give rise to powerful contractions of the bladder and rectum.
3. Irritation of the brachial, crural, median, sciatic nerves, &c., give rise to no peristaltic movements of the uterus, but only causes a slight rigidity and paleness.
4. The effect of the irritation of these nerves disappears if the aorta has been previously compressed, but irritation of the ends of the sacral nerves still causes, even after the closure of the aorta, peristaltic movements, probably through direct excitation of the involuntary muscular fibres by the accumulation of carbonic acid gas.—*British Medical Journal*, No. 717, p. 405.

(30.)—These reasonings have been confirmed by certain experiments of M. Bernard, who found that when an incision is made into a lobe of the liver in a living animal, the blood may be seen to jet from the mouths of the hepatic veins during the movements of expiration, but to return sucking in air with it at each deep inspiration, so that the animal soon dies from the passage of air into the heart.—*Dr. Charles Murchison, British Medical Journal*, No. 696, p. 567.

(31.)—Dr. Hertzmann says that by continuous administration of lactic acid to dogs and cats, rickets firstly, and then osteo-malakia may be caused, whilst in rabbits and guinea-pigs osteo-malakia may be caused without rickets occurring. Lactic acid, he says, causes rickets as long as the animal is young, and osteo malakia when it is older.—*The Doctor*, May 1st, 1874, p. 81.

(32.)—He cut down upon the splanchnics in dogs and rabbits from

behind, and divided them without opening the peritoneum. After section of both great splanchnics the blood pressure fell greatly while the rapidity of the pulse increased.

Strange to say, in some animals which survived the operation, when the blood pressure was again observed, after some days interval, it was found to have attained a degree as high as it had been before division of the nerves.

On irritating the peripheral end of the divided splanchnicus major, or the central ends of any of its roots, the blood pressure always rose, and with few exceptions slowing of the pulse resulted. The latter was much less marked when the vagi had been previously divided.—*Journal of Anatomy and Physiology*, 1869, p. 211.

(33.)—He has found that after division of intestinal nerves in dogs a large secretion of watery fluid into the intestines results. In a large healthy dog, which had fasted for twenty-four hours, he exposed a knuckle of bowel, and put four ligatures round it; these were separated from each other so that the three portions of intestine, each about fifteen centimetres in length, were included within the ligatures. He carefully isolated and divided the nerves, supplying the middle ligatured portion, taking care to avoid injury to the vessels. He closed the wound in the abdominal wall and allowed the animal to remain at rest.

In one dog a hundred grammes of fluid were found in the intestine three hours after the operation; and in another dog, killed eighteen hours after, two hundred and twenty-five grammes were present.—*Journal of Anatomy and Physiology*, 1869, p. 214.

(34.)—Mr. Joseph Michon records an experiment which produced results different from those heretofore recorded. He removed the superior cervical ganglion of the sympathetic in the common cock.—*Lancet*, No. 2,218, p. 238.

(35.)—In the last number of the American Journal of Medical Sciences we find one more added to the many theories that have been advanced to explain the uses of the cerebellum. Dr. Mitchell an able experimenter, who states he has been studying the subject for six years . . . obtains the following results in pigeons on which he had practised both the ablation of the organ and the application of intense cold by means of Richardson's spray apparatus. After ablation he found, in accordance with the observations of other experimenters, that if the wound were deep convulsions invariably occurred, together with a tendency to backward movements, and

die in an atmosphere saturated with ozone usually in about an hour and a quarter, presenting with great exactitude the order of symptoms I have described above. Rabbits live longer than guinea-pigs, exhaling water from the lungs much more freely, and also micturating with greater freedom. Rats die very rapidly. Mice exhibit a greater tolerance. Pigeons resist the effects of ozone much longer than guinea-pigs, and may be readily taken out of the chamber in which they have been confined with guinea-pigs, apparently but little inconvenienced at the period when the guinea-pigs are dead. If when just removed the chest is auscultated the breathing, however, is found to be particularly sharp, dry, and cooing, the action of the heart being amazingly rapid, reaching even two hundred and forty beats in the minute, and the respiration being from ninety to a hundred in the same time. Pigeons removed altogether from the ozone, when in this condition, will frequently exhibit the third stage of obstruction to respiration and die. Frogs seem to have the power of resisting the influence of ozone permanently, if they are allowed free access to water.—*Dr. Day, Lancet*, No. 2,317, pp. 124-5.

(38.)—In order to prove the truth of his theory he immersed two frogs in solutions of nicotine, which caused complete prostration or paralysis of motion, and the animals died paralysed in twenty-three minutes. Others he placed in solutions of strychnia, which produced tetanic effects of that drug, and caused death in four minutes. He then placed two others in a compound solution of the two poisons; the first frog lived in the compound solution forty-seven minutes, at the end of which time it died; the second frog was kept in the solution ten minutes, and then taken out, but after its removal from the liquid, in thirty minutes, it was seized with the tetanic spasms of strychnia, in the form of *emprostotonos*, but ultimately recovered.—*Dr. Part, Lancet*, No. 1,961, p. 310.

(39.)—"An experimental inquiry into the mode of death produced by aconite," by Dr. E. R. Harvey, was next read. The author's experiments, which were performed on dogs, rabbits, and frogs, went to prove that aconite acts first on the nerves and then on the muscles, killing by its action on the heart.—*Dr. Michael Foster, Lancet*, No. 1,923, p. 20.

(40.)—Frogs introduced into a weak solution containing aniline died in periods varying from a quarter of an hour to two hours and a half, and death was also caused by the introduction of aniline into the mouth or into a wound in the back. Rabbits were also poisoned by this substance, a small animal being killed by fifty drops in six hours and a quarter, and a larger one by a hundred drops in four hours. In all the animals experimented upon violent clonic and tonic spasms ensued after the application of the aniline, and continued almost uninterruptedly till death.—*Lancet*, No. 1,994, p. 470.

(41.)—Ausch finds that sensibility is so much impaired in frogs poisoned by strychnia that pinching the animal's toes, or burning the central end of the divided sciatic nerve, may be performed without being followed by a reflex action.—*Journal of Anatomy and Physiology*, 1873, p. 399.

(42.)—The four experiments now to be described are, as regards their results, merely confirmatory of those recorded by other investigators; but in their mode of performance some variations have been made from the methods usually followed, so as to show as clearly as possible the total inability of strychnia to act through the nerves. The experiments were performed on frogs, animals which possess the twofold advantage of being very sensitive to the action of strychnia, and of surviving for a considerable time arrestment of the circulation.

Experiment 1.—A ligature was placed round the heart of a frog at the auricles. A solution containing the one fourth part of a grain of strychnia was then injected into the stomach by means of a tube passed down the œsophagus. Thirty-five minutes after the injection the animal was able to leap about vigorously; fifty minutes after the injection it was still able to move its limbs, although feebly; in five minutes more all movements had ceased.

Experiment 4.—A ligature was placed round the heart of a frog at the auricles. One-and-a-half grains extract of *nux vomica*, mixed with fifteen minims of water, were then injected into the stomach by means of a tube passed down the œsophagus. Half-an-hour afterwards the animal was leaping about quite unaffected by the poison. The cranium was now cut through immediately behind the eyes with a pair of scissors, and a piece of extract of *nux vomica* was applied to the exposed nervous matter.

Experiment 5.—The apex of the ventricle of a frog's heart was cut away. An opening was then made in the anterior abdominal vein, and a strong solution of extract of *nux vomica* was injected into the vein in an upward direction, that is towards the liver and heart. . . . Twenty-six minutes after the injection the animal was able to leap about; forty-three minutes after the injection it was still able to move its limbs. In a few minutes more all movements ceased.

Experiment 6.—The greater part of the ventricle of a frog's heart was cut away. After allowing the blood to be expelled to as great an extent as possible the remains of the heart were excised. The articulation of the lower jaw on each side was then cut through in order to facilitate the next part of the operation, which consisted in cutting through the cranium with a pair of scissors immediately behind the eyes. To the part of the brain thus exposed a piece of extract of *nux vomica* was applied. In about three minutes afterwards the action of the poison began to manifest itself. The muscles

of the throat were first affected with spasm. The anterior extremities then became violently tetanised and directed upwards on each side of the head.—*Dr. Spence, Edinburgh Medical Journal*, July, 1866, p. 44.

(43.)—The following inquiry formed part of a gratuation thesis given in by me to the University of Edinburgh, on the 5th of July, 1873, entitled “Exthroxylon Coca, with an experimental inquiry into the physiological actions of cocaine, theine, caffeine, &c.” The experimental research was conducted in the Physiological Laboratory of the University, and I have gratefully to acknowledge much kind assistance from Dr. M’Kendrick.

Upwards of one hundred experiments were conducted on different living animals, chiefly frogs, mice, rabbits, and cats.

The $\frac{1}{16}$ gr. of theine, dissolved in twenty minims of water, was injected under the skin over the back of a healthy middle sized frog; almost immediately afterwards the respirations, which normally had been eighty, were increased to one hundred and twenty per minute. Seven minutes afterwards the respiration had diminished to eighty per minute.

The frog was now distinctly sluggish in its movements. It made attempts to leap, but did so feebly. When placed on its back it recovered its normal position with difficulty. When its toes were pinched with a pair of forceps it drew up its leg.

Two minutes after these symptoms were increased, and in three minutes more the limbs were very weak, and the animal lay on its belly without their support. When placed on its back the frog was unable to recover its position, but lay there with its limbs drawn up, and when the skin or toes were pinched the limbs were moved but sluggishly. The respirations had diminished to forty per minute. Five minutes later the frog still lay motionless on its back with its limbs extended. All four legs were completely paralysed, and they remained in whatever position they were placed. The frog remained in this prostrate condition for eleven minutes, when slight spasmodic movements were observed in the limbs. Four minutes later it made feeble attempts to move its legs, and when its toes were pinched it drew them up. Four minutes afterwards the animal gave a very feeble leap and tried to crawl along the table. Eighteen minutes afterwards the frog jumped readily, especially if it was irritated. It croaked vigorously when touched, and in half an hour it was apparently in its natural state with the exception of looking feeble. If smaller doses be given similar symptoms ensue, but they are less violent and less rapid in proportion to the amount.

The $\frac{1}{32}$ gr. of theine, dissolved in ten minims of water, was injected under the skin over the back of a white mouse weighing three

drachms. For fifteen minutes no effects were observed. When its tail was pinched it uttered a cry, which it did before the drug was administered. . . . It could now only crawl along the table, but could not run. . . . Five minutes later the animal lay with its limbs occasionally kicking. . . . The under surface of its body, the feet, legs, and mouth were much congested. [Duration of experiment about thirty minutes] . . . In many instances the animal had tetanic spasms and opisthotonos.

The left femoral artery of a healthy middle-sized frog was tied, and $\frac{1}{12}$ gr. of theine, dissolved in thirty-five minims of water, was injected under the skin over the back. In ten minutes the animal was almost prostrate. It lay on its back, but was still able to contract its limbs when they were pinched, which both did with equal strength, the left leg being perhaps somewhat more sluggish than the right. Fifteen minutes later the frog was apparently dead. The head was amputated; on irritating the upper portion of the cord with the electrodes of a weak Faradic current the two limbs contracted powerfully, and apparently with equal strength.

The heart of a healthy middle sized frog was exposed by carefully dividing the sternum with a pair of scissors, and a ligature passed round its back and tied so as to interrupt the circulation. The $\frac{1}{12}$ gr. of theine, dissolved in ten minims of water, was then injected under the skin of the calf of the right leg. In four minutes both of the posterior extremities were partially paralysed.

Six grains of theine, dissolved in two drachms of water, were injected under the skin over the back of a healthy white rabbit weighing 2 lbs. 3 oz. Almost immediately afterwards the ears were observed to become paler than before, then suddenly they appeared of a bright-red colour, all the vessels being enlarged and congested. After remaining in this condition for half a minute they again became paler and anæmic. These sudden changes from extreme pallor to intense congestion alternated for about five minutes, each stage being about a quarter of a minute in length, after which time the ears became permanently red, hot, and congested. The animal then became restless and somewhat excited, but not hyperæsthetic, and it trembled slightly. When its toe or ear was pinched it struggled. The force of the heart's pulsation was stronger, and the rapidity of the beats, as well as the respiratory acts, quicker than before. Pupils were unaffected. Three minutes later the hind legs struggled slightly, and they seemed to have lost power. Two minutes after all four extremities were considerably weaker, and the rabbit was unable to stand upright; but lay flat on its belly with all its limbs stretched out on the table. When its toe was pinched it did not struggle so much as formerly, still it pulled away its leg and

attempted to crawl along, which it did in a shaky and laboured manner. The breathing was laboured and slow, the heart's pulsations were feeble, and the animal trembled. For twenty minutes the animal lay in this prostrate condition, breathing in a laboured manner, when it suddenly took a tetanic spasm, with slight episthotonos, which lasted for about a quarter of a minute. . . .

For the next seven minutes the animal took tetanic spasms at intervals, occurring spontaneously, and not brought on by pinching or other external irritations.

Evidence of sensibility had disappeared from all parts of the body except the head, where it seemed to be normal.

The eyelids winked when the eyeballs were touched, and even when the hands were clapped before them. When any portion of the face was touched its muscles contracted. The animal, although completely paralysed in its limbs, looked intelligent, as if sensation was unaffected. Ten minutes later the pupils were considerably contracted, the breathing slow and irregular, and the heart-beats not palpable. It died after a tetanic spasm. The animal never seemed to lose its intelligence. It was watchful, followed every motion of the experimenter, although it was unable to make any movement.

• Six grains of theine, dissolved in a drachm and a half of water, were injected under the skin over the back of a healthy cat weighing four pounds one ounce. In ten minutes the animal became very angry and irritable. Fifteen minutes later this excitement had increased; the animal had a watchful, anxious appearance, prowled about, and when touched with a stick bit at it and growled. If any noise or motion was made it put up its back and made a hissing noise. The legs appeared weakened, and although it could still walk about, it preferred sitting in a corner of the room. Its mouth and tongue were very red, and there was an abundant secretion of saliva, which constantly trickled out of its mouth. The cat defæcated and micturated several times. Forty minutes later it continued in much the same condition. Salivation was profuse. Animal suffered from tenesmus, and it had a constant straining from the bowel of a clear fluid-like mucus. The limbs, especially the posterior ones, were much weakened, but the animal could still run with difficulty. It could not jump, it made attempts to do so over a bench about two feet high, but failed. The breathing was laboured and irregular. The redness of the tongue and mouth as well as the excessive irritability of the animal had disappeared. It was quiet, lay in a corner stupid and drowsy. It drank freely of water. Twenty minutes later it was prostrate and lay on its side, its limbs quite helpless. It paid no attention to a pinch of the toe or a blow on the tail with a stick. It seemed however to be intelligent, as its eyes watched every movement of the observer, and when the hands

were clapped before its face it growled. The salivation and discharge from the bowels were excessive. Pupils were contracted and the breathing was laboured. Five minutes later the cat took a series of tetanic spasms, and shortly afterwards died.

A healthy white rabbit, weighing two pounds two ounces, was carefully fastened down on its belly, an incision was made through the skin along the upper part of the spine, about two inches in length, and the vertebral column exposed. By means of bone forceps and scissors, portions of the vertebræ were removed so as to expose a piece of the spinal cord, about a quarter of an inch in length. On touching the posterior columns with the point of a blunt needle, the animal struggled violently and uttered loud cries. Twelve grains of theine, dissolved in two drachms of water, were then injected under the skin of the belly. In ten minutes the symptoms already described in preceding experiments commenced—congestion of the ears, &c. On pinching the toe, the animal did not appear to feel it. On touching with the point of a blunt needle the posterior columns of the cord the animal struggled, but not nearly so violent as before, and did not cry out. When the anterior columns were touched there were violent convulsions of the body. Five minutes later the animal was completely paralysed in all its limbs, and presented all the usual symptoms of prostration. A fresh portion of the cord was exposed by cutting away some of the vertebræ below the original wound. On touching as before the posterior columns the rabbit only quivered slightly. On touching the anterior columns marked muscular contraction of the limbs followed. The animal was shortly afterwards killed, and similar phenomena were observed after death as have been already described. . . . These and other experiments have frequently been repeated with various modifications. . . . The research however is yet in its infancy; and this contribution, I trust, may be looked upon as a stepping-stone to further inquiry.—*Dr. Alexander Bennett, Edinburgh Medical Journal*, 1873-4, pp. 323-34.

(44).—As regards the physiological action of caffeine he coincides with previous experiments in considering that it causes increased reflex excitability and tetanus, the action resembling that of strychnia. If, however, one sciatic nerve be divided before the poisoning, that limb is not convulsed; hence it acts on the nerve centres. A frog is tetanised by the subcutaneous injection of a quantity not exceeding 0.005 of a gramme. 0.120 of a gramme injected into the jugular of a rabbit tetanises it, and a cat or dog is tetanised by 0.2 of a gramme. It is remarkable that by maintaining artificial respiration for some time the symptoms of tetanus entirely disappear. Its action on the heart of animals is that it causes acceleration of the beats with diminution of the blood pressure; this last effect he attributes to the poison paralysing the ganglionic nerves of the heart.—*Lancet*, No. 2551, p. 92.

(45).—Dr. Foster having received a small quantity of the alcoholic tincture and glycerine solution of the alcoholic extract of jaborandi, placed the drug in my hands, and requested me to observe its physiological action. I have been able to make some observations which are confessedly very incomplete; but I am induced to publish them, since they seem to have some interest, and because I shall be quite unable to resume them until after the close of the term, it being practically almost impossible for me to pursue any serious investigation while the class teaching is going on.

1. *Effects on the general nervous system.*—Injected beneath the skin of the frog jaborandi causes tetanic convulsions not unlike those of strychnia. These continue when the brain has been removed, but do not appear after destruction of the spinal cord. [Several other experiments are described by Mr. Langley.] At page 543-4 of the same journal may be read a report of the effects of this drug on man, by a medical practitioner, who says: "We have carefully watched the effects of the jaborandi on the circulation, and always find that it increases the frequency of the pulse, and, except when the pulse is frequent from fever, we have never seen this drug lessen the frequency of the beats. Mr. Langley finds that jaborandi reduces the frequency of the heart's action in animals; hence we must conclude that, in this respect, it affects man differently from animals."—*British Medical Journal*, No. 738, p. 241; and No. 747, pp. 543-4. [See also *Harley*, No. 97, *Marcel*, *Thorowgood*, and *Lancet*, No. 126, *Yeo*, No. 172, and *Reynolds*, No. 175, and *Moore*, No. 183.]

(46).—[The following was a public exhibition:—]

Dr. Sanderson then exhibited a dog, into the abdominal cavity of which six drops of pyæmic transudation liquid had been injected three hours before. The animal was in a state of profound collapse, accompanied with vomiting, purging, and cramps of the extremities. Shortly afterwards the animal was killed and the abdominal cavity opened. The peritoneum contained liquid slightly stained with blood, which, on microscopical examination, was found to be crowded with bacteria. The intestines were distended with a frothy liquid, which possessed none of the characters of the natural contents which had been found in other cases to be charged with shed epithelium. The internal surface of the whole of the alimentary canal, from the stomach downwards, was intensely injected, and presented appearances which (as had been found by more careful investigation in previous cases) were due to the separation of the epithelium from the surface of the mucous membrane and the infiltration of that tissue with liquid.

The material which produced these results was obtained as follows:—Pus from a pyæmic abscess of spontaneous, *i. e.*, accidental origin, was introduced into the peritoneal cavity of a guinea-pig, and allowed to remain there for two days. It was then withdrawn from

the guinea-pig, and some of it at once injected into the peritoneum of a dog. The dog was affected in exactly the same way as the animal exhibited to the society. The remainder of the liquid was kept for five weeks in hermetically-sealed tubes, after which six drops were injected into the peritoneum of a guinea-pig; this showed its action to have become relatively feeble. After two days (the day before the meeting) the transudation liquid produced was tested with a third guinea-pig, and found to be extremely active. On the afternoon of the meeting it was injected into the peritoneum of the dog exhibited.

After the experiment Dr. Crisp said he differed from Dr. Sanderson in several of his conclusions. There was a great difference between tubercle in man and that in the lower animals: he had shown long ago that in monkeys and other animals no bleeding occurred from the lungs, that cavities were comparatively rare, that the liver and spleen (rarely affected with tubercle in man) were frequently so in the lower animals, and that in other particulars there were important differences. He (Dr. Crisp), as shown in their "Transactions," had inoculated guinea-pigs with pure pus from a whitlow on his own finger, and had produced tubercle, but in many respects it differed from tubercle in the human subject, although microscopically it was the same. He had also performed numerous inoculations in birds and in other animals, the results of which led him to the same conclusion. It was also important to bear in mind that there was a wide distinction between diseases of the lower animals and those affecting the human subject.

What we wanted were experiments to ascertain whether by the use of chemical agents we could render this poison innocuous—whether by any external application we could prevent the occurrence of pyæmia. Again, as regards bacteria being the cause of pyæmia, he entirely differed from Dr. Sanderson. Bacteria were found in numerous diseases of the lower animals, as had been fully shown by the French pathologists, and they were probably the effects and not the cause of the disease, as he (Dr. Crisp) had long since endeavoured to show. He was the first in this country to describe splenic apoplexy, and to point out its deleterious effects upon man and other animals. Experiments had been made by French pathologists, who showed that after the inoculation of rabbits with the blood of animals dying of this disease, that bacteria were found in the blood after a certain time, and that death took place at a given period. Dr. Sanderson's experiments appeared to him to have no important bearing upon pyæmia, the irritating matter he used acted more like a poison, such as prussic acid or arsenic, and killed nearly in a definite time as others had shown.

Mr. Hulke asked Dr. Sanderson if he had not confounded pyæmia and septicæmia. It seemed to him that the dog suffered from the latter, and not from pyæmia. If perfectly-filtered pus were injected

into an animal the ordinary symptoms of pyæmia were produced, and the animal recovered; but if the pus were unfiltered, these symptoms were produced plus others—such as multiple abscesses, constituting septicæmia. Any putrescent fluid or animal vegetable would produce the same result.

Dr. Murchison had often had the opportunity of examining the bodies of patients who had died of pyæmia following typhus fever, in which there were no ulcerated surfaces, no bed-sores, and no open wounds whatever, and yet in these pus had been deposited in the joints, under the skin, and sometimes in the internal organs. Everyone who had observed epidemics of typhus fever must have seen that pyæmia followed many cases, and that when one case had occurred in an hospital there were many.—*The Doctor*, 1872, p. 132-3. [See also pp. 24 and 39.

(47.)—The following experiments will show however that all these substances, when introduced directly into the blood, give rise to analogous reactions, or are followed by the same series of physiological phenomena, the only difference in them being in the quantity of the different salts required to produce these reactions.

In the following experiments the tubes by which the substances were injected directly into the blood-vessels, being connected either with the jugular vein or the axillary artery hæmadynamometer, when used, was connected with the femoral artery. The pressure is given in inches of mercury. In order to ascertain the general symptoms the animal was left at liberty, the substance being injected through a tube inserted into the jugular vein.

Salts of Magnesia.

Experiment 1.—The animal was a strong healthy dog weighing about eighteen pounds; six grains of sulphate of magnesia, dissolved in half an ounce of warm water, was introduced into the jugular vein.

In ten seconds the pulsations of the heart were quickened.

In five minutes the action of the heart and the oscillations were the same as before the injection.

Injected sixteen grains; in seven seconds the respiration was affected, being deeper; in ten seconds the heart was affected as before.

In forty-five seconds after the injection the animal appeared to be uneasy; respiration more laboured.

After five minutes injected twenty-five grains; in seven seconds the respiration deeper. In thirty seconds partial spasmodic contraction of muscles; . . . heart's action quick, oscillation slight. Sixty grains in three ounces of water arrested the action of the heart in eight seconds.

Only one or two respiratory movements took place after the arrest of the heart's action.

Experiment 2.—The animal was a healthy dog weighing sixteen pounds. A tube was inserted into the right axillary artery, the point directed towards the aorta, the pressure was taken in the femoral; sixteen grains of sulphate of magnesia was injected into the axillary artery. Violent struggles commencing immediately after the injection prevented the direct effect on the pressure in the arteries being observed. One minute, animal quiet, pressure one inch lower, oscillation not so great. In five minutes all effects of injection appeared to have passed, except that the pressure in the arteries was rather lower.

Injected thirty grains, apparently considerable pain, and in forty seconds a general spasmodic contraction of the muscles; this soon disappeared, and the pressure in the arteries was rather lower.

The animal after a few minutes appeared not affected.

Sixty grains injected into the artery arrested the respiratory movements, in seven seconds there was a state of general tonic spasm, and in a minute and a half the animal was dead.

Experiment 3.—Dog weighed ten pounds, not confined; eight grains of the salt was injected into the jugular; ten seconds, slight dyspnoea; twelve seconds, animal fell down as if suddenly paralysed. It got up almost immediately and walked about two seconds vomiting. After this the animal seemed not at all affected.

Fourteen grains injected; twelve seconds, animal fell on its side, legs extended, no spasm; the legs remained in any position they were placed; no expression of pain, although the animal was perfectly sensible, respiration regular.

After remaining on its side almost motionless for ten minutes, the animal rose and walked about, its movements were unsteady. Thirty-five grains arrested the action of the heart. Animal fell in twelve seconds, in thirty seconds respiratory movements arrested, animal dead.

Salts of Zinc.

Page 203. Experiment 4.—The animal was a strong healthy dog weighing fifteen pounds. A solution containing three grains of sulphate of zinc was injected into the jugular; in ten seconds the respiration was affected, and the action of the heart rendered slower; in fifteen seconds the pressure in the arteries began to fall, and in thirty-five seconds it had diminished
After five minutes the pressure was still down
Oscillation slight.

Injection six grains; no apparent immediate effect on the action of the heart, but in fifteen seconds the pressure in the arteries began to

fall, and in thirty seconds it was only equal to two inches. the respiration was irregular; one minute, efforts to vomit; two minutes, heart stopped, but respiratory movements and efforts to vomit continued; one minute after the pulsations of the heart had apparently ceased, at least the pressure in the arteries had sunk to zero.

Experiment 5.—A solution containing six grains of sulphate of zinc was injected into the axillary artery of a dog weighing ten pounds. There was immediate expression of pain and partial spasm; forty-five seconds, the animal quiet, the pressure in the arteries was about one inch lower, oscillation not so great; two minutes, vomiting.

Injected fifteen grains; five seconds, respiration suspended, spasm, retraction of head; animal died in two minutes. The heart continued beating three minutes after respiration was suspended, and after the thorax was opened.

Experiment 6.—Dog weighing eight pounds, it was not confined. Inject three grains of sulphate of zinc into the jugular; no marked effect. Inject six grains, twelve seconds after the injection the animal fell down and lay on its side perfectly powerless, no spasm; forty-five seconds, efforts to vomit; urine and fæces passed; the animal lay for some minutes perfectly still, the respiratory movements became slow and weak, so that its pulsation could not be felt through the chest, but pulsation in the femoral; sensibility unimpaired; no expression of pain. After ten seconds inject three grains; fifteen seconds, some slight movements, as if the animal was uneasy; respiration slower, ceased at two minutes three seconds; no convulsions; on opening the thorax the heart was found pulsating slowly but rhythmically.

Experiment 7.—A strong solution of the salt was injected into the jugular of a dog weighing twelve pounds; in seven seconds the pressure in the arteries began to sink; in about forty-five seconds was at zero; no pulsation of the heart after seven seconds. Animal dead one minute thirty seconds.

Page 204. Experiment 8.—Salts of manga. Dog weighed eleven pounds. Injected five grains of sulphate of manga; ten seconds, heart's action affected oscillation less; no sign of pain; pressure diminished one inch; one minute thirty seconds vomiting.

Inject ten grains; heart stopped ten seconds; animal died one minute forty-five seconds. On opening the thorax heart still.

Experiment 9.—Dog nine pounds, not confined. Inject three grains into the jugular. Twenty seconds animal fell on its side. Again rose and walked about, lay down again in two minutes;

lay perfectly still in the same position for ten minutes, although it could walk when roused. It remained five minutes without moving, the head and thorax resting on the ground, the back part of the body supported by the hind legs; there appeared a total absence of volition; no expression of pain, no convulsions. Inject four grains; animal dead in two minutes.

Experiments 10.—Salts of cobalt. Dog weighed 13 pounds. Inject five grains sulphate cobalt. Twelve seconds heart affected; fifteen seconds respiration rather deeper; the pressure diminished; at three minutes it had fallen three inches in efforts to vomit. After ten minutes the animal appeared not to suffer. Inject ten grains; blood in tubes coagulated, but in three minutes the pressure was 2.5' inch, it having been before the first injection 8.9' inch; respiration slow, heart's action weak.

Inject ten grains; in ten seconds heart stopped; respiration continued two minutes longer. On opening the thorax the heart was still irritable but did not contract rhythmically.

Experiment 11.—Dog fifteen pounds, unconfined; four grains sulphate of cobalt was injected into the jugular; forty-five seconds the animal lay on its side; respiration rather slower; three minutes vomiting. At the end of half an hour the animal still disliked moving.

Inject ten grains; prostration complete; animal lies like a dead mass; respiration slow, regular action of heart weak. After ten minutes it could stand. Inject 15 grains; ten minutes the animal fell on its side, head drawn back, struggled a little; respiration stopped two minutes. On opening the thorax the heart was found motionless.

Experiment 12.—Salts of nickel.—Dog, weight twenty pounds, pressure five to seven inches. Inject into jugular three grains of sulphate of nickel; ten seconds heart affected, quicker, oscillations less; forty-five seconds pressure 4.2' inch; two minutes efforts to vomit.

Inject six grains; twelve minutes heart quicker, respiration deeper. Two pressure three inches; respiration quiet and slow; pulsations fifty-eight. Animal appears not to suffer; lies quiet.

Eight minutes inject nine grains; heart stopped in fourteen seconds; respiration continued 2.30. On opening the thorax heart moved slightly.

Experiment 13.—Dog weighed twelve pounds, not confined; three grains of sulphate of nickel was injected into the jugular; no immediate effect; three minutes vomiting; seven minutes inject six grains; animal fell down; thirty seconds no expression of pain, no convul-

sion; breathing regular, slow, sensibility unchanged; remained in this state for some time without the slightest voluntary movement.

Inject ten grains; heart stopped; animal dead in two minutes.

Experiment 14.—Salts of cadmicun.—Dog weighed thirty pounds, unconfined; one grain of sulphate of cadmicun in two ounces of water was injected into the jugular; no appreciable symptoms. Inject two grains, thirty minutes animal appeared dull; one minute thirty seconds vomiting; five minutes inject three grains; forty-five seconds vomiting renewed; four minutes animal fell down, lay like a dead mass; respiration regular but slow; no symptoms of pain. On being placed on its feet it would stand for a few seconds, but then gradually sunk down. Inject six grains; respiration stopped in one minute forty-five seconds; on opening the thorax heart still.

Experiment 15.—Salts of copper.—Dog, weight fifteen pounds; inject three grains sulphate of copper into the jugular; twelve minutes the action of the heart affected.

Two minutes heart slower. Inject six grains, ten minutes heart fluttering for a few seconds; forty-five seconds, pulsations slower; from eighty-four to fifty-five; respiration slower; pressure in arteries diminished two inches; efforts to vomit. Inject fifteen grains; twelve seconds heart stopped; respiration continued thirty seconds, then arrested for forty-five seconds, then recommenced and continued slowly for forty-five seconds, the heart apparently beating feebly, although no effect was produced on the pressure in the arteries. Three minutes animal dead; on opening the thorax the heart was still.

Experiment 16.—Dog, weight eighteen pounds; a solution containing two grains sulphate of copper was injected into the axillary artery; ten seconds general tonic spasm, which lasted forty seconds; respiration then recommenced and continued about one minute; the pressure in the arteries fell gradually, the heart being stopped by asphyxia.

On making an incision into the parietes of the thorax three minutes after the animal had been to all appearance dead, a full inspiratory movement of the parietes of the thorax and of the diaphragm took place.

Experiment 17.—Dog weighed twelve pounds, not confined; inject three grains sulphate of copper into the jugular; thirty-five seconds the animal seemed uneasy, but no expression of pain; respiration rather deeper; two minutes vomiting; five minutes inject four grains; twelve seconds respiration deeper, more laboured; twenty seconds animal lay down, stretched out its legs and cried; no convulsions, sensibility unimpaired; two minutes rolled over on its back; three minutes respiration short and quick; four minutes efforts

to vomit, and the animal rose for a short time, but soon fell again; six minutes, inject four grains; twelve minutes respiration deeper; forty-five seconds respiration stopped; no convulsive movements, but perfectly still. One minute thirty seconds respiration again began, and continued at intervals for one minute thirty seconds; eyes closed on irritating conjunctiva two minutes after injection. On opening the thorax the ventricles were found contracting slightly.—*Journal of Anatomy and Physiology*, 1870, pp. 201 and following pages.

(48.)—Rabbits.—The subcutaneous injection of three grains of bromal hydrate produces the following effects on a rabbit of three or four pounds weight. During the first three or four minutes nothing is observed. The animal then becomes restless, and moves about quickly, rubs its nose with its fore-feet, the vessels in the ear become dilated and full of blood, the pupil contracts quickly, but at an equal rate, from its original diameter before injection of 6MM, or 7MM, to 1MM, that is, to its maximum X of contraction. In one or two minutes more the blood-vessels of the conjunctiva, more especially on the inner surface of the lids, are injected, and there is great excess of lachrymal secretion, the mucous membrane of the mouth and nostrils also becomes red, and a profuse secretion pours from the mouth. The amount of secretion is often so great as to endanger the life of the animal from suffocation, and it may be collected easily in a watch-glass.

The respirations become gradually more rapid until they are nearly double their normal amount. The cardiac pulsations are in the first instance diminished in number, afterwards they become more rapid, and they may become double the normal number. Towards the period of death they are much reduced both in number and in force. The animal then lies flat on its abdomen and chest, with the fore and hind limbs in spread-eagle fashion. There is paralysis of both fore and hind extremities. There is no hyperæsthesia so far as can be determined by pinching or by weak induced currents. In a few minutes more the head sinks on the table, the respirations become slower and more laboured, the heart beats less rapidly and more feebly, muscular twitchings are seen, and after a few clonic spasms the animal dies, frequently in a state of episthotonos. This is the general picture of the action of a minimum lethal dose which kills in from one to two hours. With a larger dose the symptoms are more intense. The animal becomes much excited, the pupil contracts with great rapidity to its smallest diameter, and after screaming for a few seconds as if in great pain, it dies in convulsions.

With a smaller dose the phenomena just noticed follow consecutively, but the symptoms are less acute, and after a period of stupor the rabbit may recover.

A dose of five grains produces great distress within four or five minutes. The animal is excited and appears to suffer acute pain. This condition is coincident in time with the contraction of the pupil and the dilatation of the blood-vessels. During the first four or five minutes the animal is quite able to run or leap, but at the end of that period there is staggering, and very soon the power of voluntary movement is lost.

When the chord has been involved several convulsions ensue. During the paralytic condition the nerves are still sensitive to very weak induced currents. The sensibility and conducting power appear to be intact. The action is therefore on the cerebral and spinal centres, and not on the peripheral terminations of the nerves.—*Dr. McKendrick (Physiological Laboratory, University of Edinburgh), Medical Journal*, July, 1874-5, p. 2.

(49.)—Note.—Since the above was written I have experimented by injecting acetic acid into the pleural sac of the horse. The experiment was made more for the purpose of determining the rapidity with which false membranes may form in the chest in pleurisy, and the results are as follows :—

Before the experiment the temperature was 96.4° , pulse forty, and respirations twelve. The acid was injected at 10.35 a.m. At 10.50 the animal was restless, with a pulse of forty-eight. At 11.15 there was observed twitching of the superficial muscles of the right side with slight gastric tympany and a pulse of fifty-two. At 12.0 the temperature was 101.4° , and now there was a slight abdominal breathing, with a visible line of demarcation, while the pulse was fifty-four and hard. The animal remained in this condition for some time, and at 8.0 p.m. it was found that the pulse had risen to 105° . On the following day (morning) those symptoms had passed off. The animal was slaughtered forty-eight hours after the injection. A post mortem having been made there were found bands of lymph, partly organised, on the pleural surface, hydrops pericardii, the quantity of the serum taken from the pericardium being about a quart. The pericardium and endocardium were both highly injected.—*The Principles and Practice of Veterinary Medicine*, by William Williams, M.R.C.V.S., F.R.S.E., &c. p. 336.

(50.)—To ascertain this they tested the first material ejected in vomiting in a number of dogs into whose veins tartar emetic had been injected, and found that it always contained some of the anti-monial salt. The fact that efforts to vomit may still occur after the stomach has been excised they do not regard as at all opposed to their view, since the salt may act on the peripheric extremities of nerves supplying the cesophagus or intestine, which also may have the power of inducing vomiting reflectorially.—*Lancet*, No. 2,561, p. 463.

(51.)—To prevent coagulation of the blood, Dr. Braxton Hicks has recommended the addition of a small quantity of phosphate of soda (Guy's Hospital Reports, 1868, p. 14) and Dr. Richardson minute quantities of ammonia. Even in very small proportions the ammonia not only prevents or retards coagulation, but in transfusion it acts as a stimulant to the system. Great care is required in using it. Injected into the veins of a dog insufficiently diluted it produced convulsions.—*Dr. Madge, British Medical Journal*, No. 68c, p. 45.

(52.)—The experiments made at Norwich by Dr. Magnan were similar to the following described by himself several years ago. The dangers of prolonged indulgence in absinthe drinking have been pointed out by many writers, and recently experiments have been made to ascertain the nature of the poisonous action of this substance. Drs. Magnan and Bouchereau add some facts to what is already known (*Comptes Rendus*, 5 Avril. 1869); they administered the poison to dogs, cats, rabbits, and guinea pigs, and found that convulsions of an epileptic character were quickly produced. The convulsions, they further show, are caused by some component part of the artemisia absinthium and not by the alcohol in which it is dissolved.—*Journal of Anatomy and Physiology*, 1870, p. 313.

(53.)—In order to compare the capability of resistance in inflamed parts with that in the normal condition, v. W. produced inflammation of the mesentery of the frog by means of cantharides. The animal was injected next day, and always at the same time a sound one for comparison. The injected matter consisted of water, soluble Berlin blue, and gelatine; and the injection was made by means of Hering's apparatus with constant pressure.

The result of seventy experiments was, that as well in normal as in inflamed mesentery, the injected mass passed through the walls of the vessels.

Similar results were obtained by another series of experiments, where the frog pumped the mass into the vessels by the action of its own heart.—*Journal of Anatomy and Physiology*, February 1875, p. 228.

(54.)—Vulpian exposed the heart of a curarised dog, and in order to diminish the rapidity of its movements, a quantity of infusion of digitalis was injected into the femoral vein.—*Journal of Anatomy and Physiology*, February 1875, p. 230.

(55.)—The animals used for experiment were the dog, the rabbit, the sheep, and the frog. Into the dog were transfused the blood of

the sheep, of the cat, of the guinea pig, of man, the frog, the calf, and the pigeon; into the rabbit that of the hare, sheep, calf, and man; and the blood of man into the sheep. A special series of experiments was made with frogs, which were injected with the blood of the dog, rabbit, sheep, man, calf, guinea pig, pigeon, and pike. The blood of the *rana temporaria* was also injected into the *rana esculenta*. The result of these painful experiments are given thus:—

“In many kinds of blood also, when mixed with the blood of the serum of other species, the blood corpuscles are seen to run together into masses; and these masses when transfusion is made into the venous system, may block up the pulmonary capillaries and give rise to very formidable symptoms.” Dr. Landois remarks that numerous experiments with various modifications are required for the solution of the questions that await an answer.—*British Medical Journal*, No. 687, pp. 280–1.

(56.)—Dr. Guérin has repeated an experiment which was performed by Blundell and others long ago, a sort of double transfusion. Two dogs are placed side by side and the carotid artery of each animal connected by means of a tube with the jugular vein of the other. In this way they obtain a common circulation, after the fashion of the Siamese Twins. Although a most interesting experiment, this can only be regarded as one of the curiosities of transfusion.—*British Medical Journal*, No. 680, p. 44.

(57.)—*The poison of some Indian venomous snakes administered to dogs, &c., by Drs. Brunton and Fayer.*

The general symptoms are depression, faintness, hurried respiration and exhaustion, lethargy, nausea, and vomiting. In guinea pigs and rabbits peculiar twitching movements occur which seem to represent vomiting in them, and occasionally in fact, guinea pigs do vomit. Dogs vomit, are salivated and present an appearance as if the hair had all been rubbed the wrong way, “staring.” As the poisoning proceeds paralysis appears, sometimes affecting the hind legs first and seeming to creep up the body, and sometimes affecting the whole animal nearly at the same time. There is a loss of co-ordinating power of the muscles of locomotion.

Hæmorrhage, relaxation of the sphincters, and involuntary evacuations, not unfrequently of a sanguineous or muco-sanguineous character often precede death, and it is generally accompanied by convulsions.

In fowls the appearance is one of extreme drowsiness; the head falls forwards, rests on the beak, and gradually the bird, no longer able to support itself, rolls over on its side. There are frequent startings, as if of sudden awaking from the drowsy state. . . .

Experiment 1.—1.30. Three drops of this diluted with water were

injected into the flank of a small dog. Immediately after the injection the corresponding leg was drawn up partially paralysed.

1.32. He walks less steadily. Tail rigidly held out.

1.35. Is restless and whining. Walks about and then sits down again. Walks unsteadily.

1.45. There are distinct muscular twitches in the shoulder. General tremor.

1.47. There are twitching movements of the back.

2.8. Has been standing perfectly still. Is now pawing and licking his lips. Vomits.

2.10. Vomits again, but licks up part of what he had ejected.

2.22. Has been continually vomiting. The ejection consisted at first of food, afterwards of tenacious mucus. He now lies down apparently exhausted. He is still trying to vomit but can bring nothing up. He tries to rise, but cannot. Convulsive struggles occur.

2.25. Breathing is ceased, but the cornea is still sensitive. Convulsive attempts to vomit.

2.27. Cornea insensible. Heart is still beating strongly. Death soon followed.

Experiment 2. A young rabbit weighing 900 grammes was used. An incision had been previously made through the skin of the neck, and the wound again sewn up, but the animal was otherwise uninjured. Two drops of cobra poison weighing 12 centigrammes were diluted with one cubic centimetre of water.

At 4.6. The diluted poison was injected under the skin of the left hip.

4.7. Washed out the watch glass in which the poison had been placed with water, and injected it under the skin of the back. The animal sat quiet after the injection, occasionally licking its fore paws.

8' 30". Respiration seems hurried. The rabbit occasionally makes a jerking motion with its hind feet.

10'. Has been restless, running about occasionally licking its fore feet.

13' 30". Still very restless and when held makes convulsive efforts to get away. Ears are much congested.

17'. The animal is now quiet, its ears are no longer congested.

About 20'. Quiet with occasional starts. Disinclined to move but can walk quite well.

25'. Movements seem difficult and hind legs seem weak when it tries to walk.

26'. Paralysis of hind feet is increasing.

26' 15". The rabbit lays its head down on the table.

28'. When laid on its side it merely makes a few slight movements with its fore paws, and then lies still. The eyes remain in a half-closed condition and have done so for some time. When the cornea is touched the head gives a jerk, but the eyelids move very little. Respiration slow and laboured.

4.30. The chin is twitched inwards, the sterum once or twice, the hind feet at the same time being twitched backwards. The eyes open widely. Slight convulsive extension of limbs.

4.31. Respiration has stopped, cornea is insensible; thorax opened immediately. There were large extravasations of blood under the skin of abdomen and thorax, and under the skin of the left hip. Heart beating vigorously.

The muscles contracted on direct irritation. The foot twitched when the sciatic nerve was exposed and irritated by an interrupted current. The peristaltic movements of the intestine were active after the abdomen was opened.

Experiment 3.—Dissolved five milligrammes of dried cobra poison which had collected round the stopper of the bottle containing it in one and a half cubic centimetres of water, and injected it under the skin of the left hip of a guinea pig weighing 790 grammes.

In three-quarters of a minute after the injection the animal became restless and uneasy and began to cry.

1½'. It began to give little starts.

3¼'. The starting motions became greater, the hind quarters of the animal being jerked upwards and the chin drawn in towards the body; continues to cry.

4¼'. Passes water.

7'. Less restless.

15'. Washed out the watch glass in which the cobra poison had been placed with about half a cubic centimetre of water, and injected it as before. Immediately afterwards the restlessness increased.

24'. Seems to be trying to vomit.

27'. It cannot walk rightly.

28'. The hind legs are paralysed and spread out laterally from beneath it.

29'. Respiration very slow and deep. The animal lies quiet, but convulsive twitches of the limb follow almost every respiration. Respiration eight in half a minute.

30'. Cornea insensible. Respiration has ceased. Post-mortem examination made immediately. The left ventricle was much dilated, the right ventricle empty. There were two beats of the left auricle for every one of the ventricle, and the ventricular beat was weak and imperfect.

Experiment 4. Dissolved one centigramme of a substance like gum, and labelled "alcoholic extract of cobra poison" in one cubic centimetre of water. It dissolved easily and formed a somewhat opalescent solution.

Injected about one-third of this (equal to three and a half milligrammes of the dried extract) under the skin of the thigh of a rabbit weighing about a kilogramme. Four minutes after the injection there was no apparent effect; so a similar quantity was again injected, making the total amount received by the rabbit seven milligrammes

of the extract; five and a half minutes after the first injection the animal became very restless.

7'. Respiration rapid. The vessels of the ears were noticed to be much injected. On continuing to observe them the injection disappeared and then returned again. The alternate filling and emptying of the vessels was much more perceptible than in the normal condition. The rabbit sits quietly, but every now and then gives a start.

22'. The condition of the ears has continued the same. The eyes are becoming half-shut and the eyeballs turned up.

The animal now begins to tremble. The head is laid down on the table and then raised again; this is succeeded by a nodding motion of the head. The head is next laid down on the table. Respirations twenty-two in fifteen seconds.

24'. The animal has sunk down on its face and paws as if its fore-legs would no longer support it. The hind legs, however, still support the posterior parts of the body. Respirations eleven in ten seconds. It seems to be trying in vain to raise its head.

26'. Respirations eight in ten seconds. Convulsions. The cornea is sensitive. The rabbit is now lying on its side. Respirations five in fifteen seconds. Pulse twelve in eighteen seconds.

31'. Cornea is nearly but not quite insensible. The eyeball was protruding.

About 31½'. Respiration has stopped. The heart is still beating vigorously.

32'. Cornea insensible. The animal opened immediately. The heart was beating vigorously, twenty-one beats in ten seconds.

An attempt was made to insert electrodes into the spinal cord and pass uninterrupted currents through them. No effect followed; but it is not certain that they were well in the cord. Irritation of the nerves going to the hind legs by uninterrupted current had but a slight effect. Direct irritation of the muscles caused them to contract. After the irritation was discontinued, a fibrillary twitching was observed in one of the extensions of the thigh.

42'. Heart still feebly pulsating. Irritation of the brachial sciatic and crural nerves has very little effect.

45'. Heart still feebly pulsating.

Experiment 5.—Two drops of cobra poison were injected under the skin of the thigh of a guinea pig. One or two minutes after the injection the legs of the animal began to twitch. It was then covered with a glass bell jar.

6'. After injection. The legs are again twitching. This is a peculiar motion of the hind legs in which they seem to make an abortive attempt to kick involuntarily.

7'. Respirations are deeper than usual.

9'. Legs again twitching.

10'. The animal is restless and moves round and round inside the

bell jar. Grunts occasionally and grinds its teeth. The hind-quarters are twitched upwards and the nose is drawn in towards the chin at the same time.

13'. Bites at the spot where the injection was made and passes water.

22'. It can no longer walk.

23'. It has sunk down and lies flat on the table leaning rather to one side. Respirations are deep. There are occasional twitches of the legs.

25'. Cornea is sensitive. Occasional convulsive stretches.

27'. Cornea almost insensible. Respiratory movement of nostrils continues.

28'. Cornea completely insensible. Post-mortem examination made immediately. The muscles of the abdomen were dark coloured. Peristaltic movements of the intestines occurred when the abdominal cavity was opened. The heart was dark and slightly dilated; all its cavities were contracting though feebly. There were three beats of the auricles to each one of the ventricles. Irritation of the nerves in the pelvis caused contractions of the legs.

35'. After injection. The heart is still feebly contracting.

Experiment 6.—October 28th. Injected about a grain and a half or two grains of the precipitate which was thrown down from cobra poison by alcohol into the thigh of a guinea-pig.

2.30. Injection made. A few minutes afterwards it passed some milky-looking water and then remained perfectly quiet.

3.8½. Passed water which was quite clean.

3.33. Injected about two grains into the right femoral vein. It passed clear water almost at once.

3.35. Its nose gave a jerk inwards. Wounded leg drawn up.

3.38. Nose twitches frequently, and the animal emits a faint barking sound.

3.40. Slight tremors.

3.50. Begins to eat a piece of bread placed near it.

3.58. Still twitches.

4.8. Is still sluggish, but seems nearly well. Recovered.

Experiment 7.—October 29th, 1872. About half a grain of fresh but coagulated and cheese-like cobra poison was suspended in distilled water and injected into the back of a guinea-pig weighing about a pound and a quarter.

2.23. Injection made.

2.26. The animal looks scared and is twitching. This guinea-pig is very active.

2.30. Another dose injected. The animal is twitching much. It jumped out of the deep box in which it had been placed for observation. Breathing is hurried.

2.36. It seems better. Another dose injected into the thigh.

2.45. Not much effect. Another dose injected.

2.46. Twitching continues. Animal remains active. It recovered.

Snake poison probably produces its fatal or deleterious effects either by completely paralysing the nerve centres or other portions of the nervous apparatus, and thus causing arrest of respiration, or by partially paralysing them and also poisoning the blood, thereby inducing pathological conditions of a secondary nature, which may, according to circumstances, cause the slightest or most dangerous symptoms.—*Proceedings of the Royal Society*, Vol. xxi., No. 145. pp. 365-70.

(58.) An opportunity occurred to me in June last of performing some experiments with the poison of the cobra di capello, or naja tripudians. A small quantity of the cobra poison in a dry state was given to me by Mr. Blackburn, formerly a pupil at Guy's Hospital, . . . with the aid of my colleague, Dr. Pavy, the experiments were performed in his physiological laboratory. . . . Two grains thus dissolved, including the gelatinous portion were introduced into a wound made in the side of a rabbit by Dr. Pavy; the wound penetrated into the cellular tissue beneath the skin: very little blood escaped. The edges of the wound were then sewn up. No particular symptoms manifested themselves for a quarter of an hour. Mr. K. Ashby undertook to watch the animal, and I subjoin a note of his observations.

Began to show symptoms of loss of power in limbs, particularly in hind legs, resting its body on the table without supporting itself on its limbs. It got up and moved when irritated. Respirations quick. Cannot support itself on its legs.

Lies over on its side. Respiration slow.

Eyelid shut lazily when the cornea is touched.

Eyes insensible to the touch. Six or eight slight convulsions.

Respirations, all movements ceased twenty minutes after the poison had begun to act. Chest opened five minutes afterwards Heart still beating, and continued to beat for five minutes after the chest was opened, when it became engorged and stopped.

A middle-sized healthy dog was selected for the next experiment. The animal had been kept without food for many hours. Two grains of the poison, finely powdered and mixed with a small quantity of powdered gum, to give it uniform consistency, was brought to a fluid state with half an ounce of water. This was injected into the stomach of the animal by means of a catheter.

No symptoms of poisoning were at any time observed, and in the following day the dog was as well as usual, and took his food with appetite.—*Dr. Taylor, Guy's Hospital Reports*, 1873, p. 297.

(59.)—About two hundred and eighty experiments are recorded. per-

formed by Dr. Fayrer, on dogs, cats, pigs, kids, birds, rats, horses, and several other animals, consisting of various snake bites. It is needless to add that the animals suffered much pain for periods of from one to several hours duration—in some instances seventy hours.

Nine experiments were performed on dogs, pigeons and fowls.—*Edinburgh Medical Journal*, Vol. xiv. p. 522.

Twenty-seven experiments were performed on fowls, a dog, a cat, frogs and fish.—*Ibid.*, pp. 915-21.

Nineteen experiments were performed on dogs, cats, birds, a pig, &c., &c.—*Ibid.*, p. 996.

Six experiments on dogs, a pig, a snake, and a civet cat.—*Ibid.*, Vol. xv. p. 236.

Two experiments on two horses.—*Ibid.*, p. 242.

Fourteen experiments on fowls, cats, and snakes.—*Ibid.*, p. 245.

Thirteen experiments were performed on fowls, a chicken and a snake.—*Ibid.*, p. 334.

Twenty-one experiments were performed on snails, fowls, dogs, chickens, and snakes.—*Ibid.* p. 417.

Two experiments on snakes.—*Ibid.*, p. 423.

Four experiments on a cock, a rabbit, and a dog.—*Ibid.*, p. 427.

Five experiments on dogs.—*Ibid.*, p. 428.

Two experiments on dogs.—*Ibid.*, p. 429.

Four experiments on dogs, a kid, and a pigeon.—*Ibid.*, p. 620.

Eleven experiments on dogs, fowls, and a snake.—*Ibid.*, p. 807.

Nineteen experiments on dogs, cats, and a fowl.—*Ibid.*, p. 813.

Eight experiments on dogs, kittens and snakes.—*Ibid.*, p. 994.

Ten experiments on dogs, a fowl, and a snake.—*Ibid.*, p. 998.

Three experiments on a dog and fowls.—*Ibid.*, p. 1,000.

Six experiments on dogs and fowls.—*Ibid.*, p. 1,099.

Four experiments on dogs.—*Ibid.*, p. 1,104.

Five experiments on dogs and fowls by the cobra poison.—*Ibid.*, p. 1,106.

Six experiments on dogs and fowls by cobra poison.—*Ibid.*, Vol. xvi. p. 53.

Five experiments on dogs and fowls by cobra poison.—*Ibid.*, p. 56.

Seven experiments on dogs, cats, and a snake by cobra poison and carbolic acid.—*Ibid.*, p. 57.

Four experiments on dogs and fowls by cobra poison.—*Ibid.*, p. 135.

Four experiments on dogs and fowls by cobra poison.—*Ibid.*, p. 137.

Three experiments on dogs and fowls. Cobra poison. Dr. Fayrer says :—I have seen as much difference between the effects produced by different daboias, or by the same daboia on different animals of the same species, as in those that had been bitten by the cobra ; and on the other hand similar differences in the effects of the bites of

different cobras, or of the same cobra on different animals of the same species as in those bitten by the daboia.—*Edinburgh Medical Journal*, Vol. xvi. p. 139.

Four experiments on dogs and fowls. Cobra poison.—*Ibid.*, p. 423.

Seven experiments on dogs and fowls. Cobra poison.—*Ibid.*, p. 426.

Five experiments on dogs and fowls by snake bites.—*Ibid.*, p. 430.

Six experiments on dogs and fowls by snake bites.—*Ibid.*, p. 431.

Two experiments on dogs by snake bites.—*Ibid.*, p. 434.

Five experiments. Dogs and fowls.—*Ibid.*, p. 435.

Five experiments performed on dogs and fowls by snake bites. [One of the dogs is described as "wildly excited, whining and licking the bitten part, which is bleeding and swollen."]—*Ibid.*, p. 628.

Six experiments of much the same character as the above, the animals remaining in great suffering for from two to seventy hours.—*Ibid.*, p. 631.

The following will generally serve to describe the 280 experiments, as regards the sufferings of the animals.

No. 1.—A full grown Pariah dog was bitten in the thigh by a full grown and vigorous cobra of the variety called by the snakemen kurees keauteah. Two tablespoonfuls of Mr. Otho Alexander's fluid antidote was poured down the dog's throat immediately after the bite, which was inflicted at 12.18 p.m., and the vegetable extract or paste made into the consistency of honey with liquor ammonia, was well rubbed into the wound and over a large surface round it. . . .

12.19 p.m.—The dog limped on the bitten leg, and seemed restless and uneasy; 12.26, retching; 12.30, very restless, breathing hurried; 12.31, vomiting; 12.33, staggering, profuse defecation; 12.34, convulsed, diarrhoea; rises, staggers, and falls over into convulsions; 12.35, violent convulsions; 12.37, perfectly paralysed, heart still beats, no respiration; 12.38, dead, in twenty minutes after the bite.

No. 5.—A fowl had twenty drops of the blood of the dog, poisoned by a cobra in experiment No. 1, injected with the hypodermic syringe into each thigh at 12.42 p.m. 12.50, crouching; 12.55, crouching, feathers ruffled; 1.5, sluggish, eyes closed, drowsy; 1.30, stands with head depressed, feathers staring, eyes closed, very drowsy; 2.0, very drowsy, head drooping; after this the fowl slowly recovered, and on Monday 24th was quite well. The poison in this experiment must have been infinitesimal in quantity, only forty drops of the blood of a full grown dog poisoned by cobra were injected. The symptoms of poison were well marked, though the bird ultimately recovered.

No. 6.—A solution of one part of cobra poison to eight parts of liquor potassæ was prepared by Dr. Ewart, and of this nine drops were injected into a fowl's thigh at 12.57 p.m. There was a flocculent-looking deposit caused by the mixture of the fluids. 1.0 a.m., drooping; 1.2, crouching; head falling over, nearly paralysed; 1.4, con-

vulsed; 1.7, dead in seven minutes, with all the symptoms of cobra poisoning.

No. 8.—Twenty drops of the blood of the above fowl removed immediately after death, injected into either thigh of a fowl at 1.10 p.m. 2.15, sluggish; 4.10, drowsy, head falls over; 4.20, no convulsions; 7.15, dead in six hours and fifteen minutes.

No. 10.—A fowl was bitten in the thigh by a *Bungarus coeruleus* (Krait) at 1.22 p.m. 2.24, feathers staring, eyes have a fixed glaring stare; 1.25, stretches out the neck, falls over, point of beak resting on the ground; 1.26, convulsed, puncture in thigh ecchymosed and cedematous; 1.29, dead in seven minutes.

No. 11.—A cat was bitten in the thigh by a cobra (*Tentulia keauteah*) at 1.46 p.m. Mr. O. Alexander's antidote and extract were administered, according to his instructions, immediately, 1.47, p.m. pupils widely dilated; cat lies stretched out, hurried breathing; 1.51, convulsed; 1.52, paralysed, heart still beats, no respiration; 1.55, dead in nine minutes.

This cat was on former occasions bitten by a large *Bungarus fasciatus*, and showed no signs of poisoning.

No. 12.—A fowl was bitten in the thigh by a large *Bungarus fasciatus* at 1.44 p.m. 1.54, drooping head, falling forwards; 1.58, convulsed, cannot stand; 2.0, p.m., convulsive movements; there is a peculiar vocal sound as though the thorax was compressed; 2.5, convulsed; 2.10, dead in twenty-six minutes.

No. 13.—A young rat was bitten in the thigh by a *Bungarus coeruleus* at 20 p.m.; insensible immediately, dead in thirty seconds.—*Edinburgh Medical Journal*, 1870-71, p. 721.

(60.)—Irritation of the roots of pneumogastric, in Bernard's famous experiment of puncture of the fourth ventricle, has the same effect as stimulation of its trunk.

This experiment is performed by pushing an instrument like a bradawl through the skull and cerebellum till it reaches the olivary fasciculi in the medulla oblongata.—*Dr. Brunton, British Medical Journal*, No. 680, p. 40-41.

(61.)—For the experiments non-narcotised dogs were for the most part employed, the surface of the brain being stimulated by weak induced currents. In general, the facts already known were confirmed.

After stimulation with quite weak currents, after movements, dependent on the stimulated centre, were manifested by the muscles, which movements often passed into general convulsions, but could only be produced from the points which were to be regarded as so-called centra.—*Journal of Anatomy and Physiology*, February 1875, p. 210.

(62.)—The author's experiments upon rabbits confirms the statement of Riegel and Jolly that the vessels of the pia mater do not contract upon a stimulus being applied to peripheral sensory nerves. After an experiment upon a tracheotomised rabbit (to which no curare had been given) had shown that this poison had no effect upon the vastomotor channels; it was tried to investigate the central circulation without trepanning, and with the exclusion of air, viz.: by ophthalmoscopic examination of the retinal vessels.

The occurrence of a strong and continual flow of tears rendered every attempt at examination fruitless. Lastly, the experiments with trepanning and the setting a piece of glass in the skull seemed to be useless, in that the action of the air caused inflammation.—*Journal of Anatomy and Physiology*, February 1875, p. 213.

(63.)—Veyssiere has recently made some experiments on the localisation of sensibility in the dog. The method he employed was to introduce a puncture into the cranium. The stylet was then withdrawn, and another bearing a concealed spring, which could be thrust out at an angle from its extremity was pushed down the canula, the spring projected and, the requisite laceration made, the spring recovered, and the instrument withdrawn.

No. 1.—The lesion was limited to the posterior part of the intraventricular of the corpus striatum, and the anterior part of the optic thalamus on the left side. Imperfect hemiplegia and hemiancæsthesia of the right side. The hemiplegia passed off in twenty-four hours, the animal was well in three days.

[The other four experiments are of the same character.]—*Edinburgh Medical Journal*, January 1875, p. 659.

(64.)—By means of a sharpened spoon the outer and upper portion of the anterior lobe of the hemisphere was so far removed that the parts lying immediately outside of the anterior portion of the lateral ventricle were exposed, without however, opening the ventricle. (The anterior can be opened without injury.) The deepest part and that lying next the middle line in this prepared surface corresponds to the corpus striatum.

When the surface of the corpus striatum was stimulated electrically, it was shown—

1st. That movements of the muscles of the opposite side occurred upon the application of weak induced current.

2nd. That the points where stimulation of the intact surface of the brain was followed by distinct groups of movements are also present on the surface of the corpus striatum.

3rd. That the opposite position of the active points is the same in the corpus striatum as in the surface of the brain.

If the deepest part of the corpus striatum is stimulated the animal opens its mouth, puts out its tongue, and draws it in again alternately.

These are the movements whose centres are pretended to be found on the convolutions of the under surface of the brain, *i.e.* lower frontal and suprasylvian convolutions.—*Dr. Sanderson, Journal of Anatomy and Physiology*, November 1874, p. 209.

(65.)—In his experiments on this subject Afanasieff divided one or both peduncles through a hole in the temporal bone. In consequence of the transitory irritation which the section produced the animal immediately afterwards drew itself together; the head was inclined to that side on which the peduncle had been divided, the pupils became contracted, especially on that side, and the arteries of the ears also contracted, but their contraction was more marked in the ear of the opposite side. In six seconds after the operation all the above-mentioned effects were succeeded by their opposites. The irritation also produced increased flow of tears and saliva, and twitchings of the extremities on the side opposite the section, all of which lasted for half an hour. Section of one peduncle between the pons and the tuber cinereum produced paralysis of the muscles of the extremities on the opposite side, and of those of the back and neck on the same side. The amount of paralysis increased with time. In two or three weeks it was again able to run straight forward.—*Journal of Anatomy and Physiology*, 1871-2, p. 218.

(66.)—Dr. Schüller (Berlin) performed experiments on rabbits by removing with a trephine a portion of the upper part of the parietal bone without injuring the dura mater, and generally at the same time taking away the cervicle sympathetic with the superior cervicle ganglion of the same side. He was thus able to observe the vessels of the pia mater, and to notice any changes in their feeling. The substances with which he experimented were mustard, nitrate of amyl, ergotin, opium, and chloroform. Small sinapisms produced scarcely any effect on the vessels; the application of large ones, on the one hand, was first regularly followed by dilatation, which was followed by more or less rapidly alternating changes in the calibre of the vessels, and finally by contraction, which often continued an hour and a half after the removal of the sinapism.—*British Medical Journal*, February, 1875, p. 297.

(67.)—Injuring with a needle a certain spot on the surface of the brain of the rabbit peculiar disturbances occur; above all hæmorrhage in the lungs and in the tissues of the same, often so pronounced that almost the whole lung is traversed by the hæmorrhage.—*Journal of Anatomy and Physiology*, 1873, p. 397.

(68.)—J. Schrieber has operated on a large number of rabbits

with a view to determining the effect of injuries to the brain on the temperature of the body.

The injury was made by means of a lancet-shaped needle introduced through the skull. The temperature was measured in the rectum. From about seventy experiments the author concludes that after injury of the pons in all parts, of the pedunculi cerebri, of the cerebellum and cerebrum, increase of the body temperature occurred when the animals were protected artificially from losing warmth; that the same results followed unconditionally and constantly on injury to the limit between the medulla and the pons.—*Journal of Anatomy and Physiology*, 1873, p. 398. —

(69.)—1st. Ablation of both sphine palatine ganglia does not affect, in dogs and cats, the sense of taste in parts [supplied by the linguals.

2nd. After section of the chorda tympani, in dogs and cats with cut glosso-pharyngei, the taste was little modified in some cases, notably diminished in others, and completely abolished in one.

3rd. After section of the chorda, in cats, dogs, rabbits, and guinea-pigs, degenerated nerve fibres were found in the terminal branches of the lingual as well as in the mucous layer of the tongue and submaxillary gland.

4th. After section of the chorda in the ear, the central end of this nerve (on the side of its emergence facially) remains healthy.—*Journal of Anatomy and Physiology*, 1873, p. 398.

(70.)—Curarised dogs, in whom artificial respiration was maintained, were used for experiment.

After having exposed the glosso-pharyngeal nerve, below the base of the cranium, it was tied and then cut above the ligature, so that its peripheral extremity could be excited by electricity. The effect of the faradisation lasted some time, and could be renewed at pleasure.—*The Doctor*, June 1st, 1875, p. 105.

(71.)—No. 1. The first pigeon had had the anterior part of both hemispheres removed. The bird was able to fly and walk, and in every respect seemed to have its faculties intact, except that when it tried to peck up food it never succeeded in getting hold of a single particle. That was what he invariably observed when the anterior part of the cerebral lobes had been removed.

No. 2. Showed two pigeons with the posterior part of both hemispheres removed. These were not quite so active as the first bird. He had never seen them attempt to eat at all.

No. 3. There were here two pigeons in which the right cerebral

hemispheres had been removed. One would scarcely be able to detect any difference between them and ordinary pigeons. The removing of one cerebral hemisphere made almost no difference.

No. 4. In those classed No. 4 the left cerebral hemisphere had been removed, and the birds were both quite active. They saw on both sides, and were in the same condition as those in No. 3.

No. 5 was a bird the right cerebral hemisphere of which he had removed, along with a considerable slice of the upper part of the right corpus striatum. He had shaved off the grey matter that represented the cerebral hemisphere, and had carried the knife deeply through the substance of the brain, so that he was quite sure that he had removed a considerable portion of the right corpus striatum. It would be observed that the bird was blind on the opposite side.

No. 6 showed the left cerebral hemisphere and the left half of the corpus striatum removed. The result was exactly the same (only on the other side) as in the other case. This was a point he was very glad to be able to demonstrate, as he was somewhat doubtful about it for a considerable time. He now knew that if he removed the upper part of the corpus striatum there was loss of vision, in the pigeon at all events, on the opposite side.

No. 7 was a bird from which he had intended to remove the cerebral hemispheres only, but in the operation he unfortunately injured the upper part of the corpus striatum. The forceps with which he removed the skull-cap slipped deeply into the corpus striatum, so that the bird was much more in the condition of pigeons in which the corpus striatum had been injured as well as the cerebral hemispheres. It could fly, it could walk; he did not think it saw or heard, or if it did so it was only to a small extent. It did not manifest any sign of alarm, and if flung into the air could fly quite well. Birds in that condition very often attempted to take food, but they never pecked up anything, and birds in this condition required constantly to be fed, so that it became a serious matter to keep a number of them.

He gave the birds water, but had to introduce the beak; they dipped their heads deeply into the water, and took one drink. Where the hemispheres and corpus striatum had been removed the birds seldom took a second drink.

No. 8 showed two birds in which the cerebral hemispheres alone had been removed. By these he tried to illustrate that consciousness was apparently not entirely lost; one could still frighten the birds.

No. 9 was a bird exactly in the same condition as the previous two.

No. 10 was a bird on which he had performed the experiment he had described, of injuring the deeper parts of the corpora striata and the cerebral hemispheres by passing a long narrow knife into the side of the head and severing the connection. The corpora striata, though injured, had not been removed. The creature was in a state of deep stupor.

No. 11 was a bird in which both cerebral hemispheres had been injured but not removed. The bird was still able to fly, but he had observed that the creature had not taken any food since the operation.

No. 12. In the cases of No. 12 both birds had a large portion of the cerebellum removed, and he thought it would be found that the faculties of the birds were intact, and that they could fly and walk.

No. 13 was a bird in great health. On the 5th of March he removed from the animal considerably more than half of the cerebellum. At first the bird staggered slightly in its movements, but now it had completely recovered. Within the last six weeks it had been regaining its plumage, and was now as handsome a pigeon as one could see.

Dr. MacKendrick concluded by stating that if any wished to see him operating, he would have much pleasure in receiving a visit from them at the laboratory.—*Edinburgh Medical Journal*, 1873, pp. 652-3.

(72.)—He experimented exclusively on the brain of the rabbit. On puncturing with a fine microscopic needle a spot lying in the interior of the posterior end of the cerebrum the animal sprung from the table and exhibited unusually violent spasmodic movements, which appeared either at the time of puncture or a second or so thereafter (at the latest two minutes), and lasted not longer than three minutes.

Many of the animals so injured had meningitis with diplocu. but asphixia and death without meningitis was also observed.

More than forty experiments, in different ways and different directions, were made in the thalamus opticus. Slight disturbance of the superficial layers was without effect. In a few cases the paralysis of the extensors of the finger observed by Schiff occurred. If punctured more deeply, and towards the middle line, the limbs of the opposite side were directed towards the middle line. This was specially and almost exclusively observed of the fore-limbs. The deviation was the most pronounced the more basal the direction of the puncture. The deviation in all cases was only temporary, disappearing sometimes after a few hours, in most cases after twenty-four hours. In other cases, immediately after puncture, the head was turned to the opposite side, the fore-limbs strongly divergent, the one directed outwards, the other (opposite side from injury) towards the middle line. No disturbance of sensibility. [The editor states that these results are at variance with those alleged to be found by Fournier.]—*Journal of Anatomy and Physiology*, 1873, p. 395.

(73.)—Leyden has made a series of observations upon the move-

ments of the brain, and the blood pressure within the cranium by means of a manometer screwed into an artificial opening made in the skulls of dogs.—*Dr. Rutherford, Journal of Anatomy and Physiology*, 1867, pp. 35-89.

(74).—He gives the results of a number of experiments upon the *crura cerebelli* of rabbits. The author working under the direction of Professor Eckhard, has taken great care to ascertain the exact position and extent of the lesion which he inflicted.

He gives an exact description of his mode of operating which will prove of not a little service to those who may wish to perform experiments, whether for the purpose of research or demonstration.

In his experiments on the "tract of the *crus cerebelli*, formed by the anterior and posterior *crus*" he found that very much the same effects followed a variety of injuries to the tract comprehending punctures to the depth of one, usually two millimetres, some in its middle, some on its outer, others on its inner side. Irregular but not violent contractions followed these injuries. Voluntary movement was slightly disturbed. Some animals showed a tendency to lie upon the injured side.

In a second series of researches he studied the effect of complete division of this tract. The section was followed by irregular convulsive contractions of varying intensity, extending throughout the entire body.

Voluntary movement seemed to be entirely abolished. The animal lay upon the injured side and fell always into this position when it was placed in any other. Some animals were observed in this state for three or four hours, others died from the hæmorrhage, which the operation for exposure of the brain had occasioned.—*Journal of Anatomy and Physiology*, 1869, p. 208.

(75).—At the recent meeting of the British Medical Association, Dr. Brunton read a paper communicated by Dr. Ferrier, containing an "Abstract of Experiments on the Brains of Monkeys, with special reference to the Localisation of Sensory Centres in the Convolutions." The experiments, which were conducted by trephining and the destruction of the sensory centres by means of a red hot wire, led to the following results: These centres are bilateral, so that when one of the centres of touch was destroyed there was loss of tactile sensibility in the corresponding half of the body. Stimulation of the centre of hearing caused the animal to prick up its ears, as if it heard something, while destruction of the whole of this centre rendered the creature totally deaf. Destruction of the centre of vision corresponding to one eye (*e. g.*, the right) only rendered the

animal temporarily blind in that eye, the function, after twenty-four hours, being carried on by the opposite centre. In the discussion that followed Dr. Nairne pointed out that other observers had arrived at conclusions different from those of Dr. Ferrier, and that the brain of a monkey could not be taken as exactly similar to that of a man; but Dr. Brunton thought the mistake made by German and other investigators who differed from Dr. Ferrier was, that they took the brains of animals lower even than the monkey to correspond with that of man. M. Dupuy had arrived at different results. He said that he had found that when the centres of motion on one side of the brain were removed paralysis followed for a short time throughout the corresponding part of the body, but that when the centres were removed from both sides of the brain there was no paralysis at all.—*Lancet*, No. 2712. p. 289.

(76.)—Dr. Brown-Séquard, in a lecture before the Boston Society of Natural History, published in the Boston Medical and Surgical Journal dissents altogether from the conclusions that have been drawn on this subject, and which have lately occupied so much attention.

Our readers will remember that Fritsch and Hitzig, followed by Charcot, Ferrier, and others, concluded that the fronto-parietal convolutions of the brain are the centres for the voluntary movements of definite groups of muscles: but Brown-Séquard explains the facts in quite another way.—*The Doctor*, October 1st, 1875, p. 181.

Professor Hitzig refers to Burdon-Sanderson's experiments and remarks "that the localised points on the surface of the brain given by Burdon-Sanderson do not correspond with those described by himself."—*Journal of Anatomy and Physiology*, November 1874, p. 210. [See also p. 397.]

(77.)—*On the Suffocation of Animals.*

About the year 1860 the Royal Humane Society received several suggestions from Dr. Silvester for restoring suspended animation in persons apparently drowned. Other methods were shortly afterwards placed before the society, on the merits of which that body felt itself unable to decide, and consequently its committee desired the Royal Medical and Chirurgical Society to investigate the several proposals, without indicating the tests which that learned body should apply, or the means they should adopt. It is important to bear in mind that the Royal Humane Society is in no way answerable for the course taken by the Royal Medical and Chirurgical Society, and that they were not even aware, until the delivery of the report from which the following extracts are made, that experiments on animals had been performed by the Committee of Investigation.

Seventy-six experiments were made on animals, in only a few of

which anaesthesia was present ; and after the terrible sufferings caused by plugging their windpipes to suffocate them, holding them under water, and in some cases restoring them to life for further experimentation, burying their heads in liquid plaster of Paris or mercury, cauterising their bodies with an iron heated to a white heat, &c., &c. the committee, it will be seen, report that they were unable to recommend any material improvement in the plan adopted by the society.

Report of the Committee appointed by the Royal Medical and Chirurgical Society to investigate the subject of Suspended Animation.

Members of the Committee.—C. J. B. Williams, M.D., F.R.S. ; C. E. Brown-Séquard, M.D., F.R.S. ; George Harley, M.D. ; W. S. Kirkes, M.D. ; H. Hyde Salter, M.D., F.R.S. ; J. B. Sanderson, M.D. ; W. S. Savory, F.R.S. ; E. H. Sieveking, M.D. (ex officio).

At the first meeting of the committee it was resolved to pursue the inquiry—

By means of experiments upon living animals.

By means of experiments upon the human body.

Two sub-committees were forthwith appointed for these purposes.

Report of the Sub-committee appointed to investigate the subject of Suspended Animation by means of experiments upon living animals.

In investigating anew the subject of apnœa by means of experiments on the lower animals, it seemed expedient to observe, in the first place, the principal phenomena of apnœa in its least complicated form, viz., when produced by simply depriving the animal of air.

The following plan of effecting this was adopted :—The animal was secured on its back, and the trachea was exposed by a single incision in the mesial line of the neck. A ligature having been passed round it, it was opened by a vertical cut, and a glass tube, as large as could be conveniently inserted, was passed into it for a short distance downwards, and firmly secured by the ligature. Through this tube, while patent, the animal breathed freely, but the supply of air could be at once completely cut off by inserting a tightly-fitting cork into the upper end of the tube. It was ascertained by separate experiments that the tube thus plugged with the cork was perfectly air-tight.

The duration of the heart's action was conveniently ascertained by means of a long pin inserted through the thoracic walls into some part of the ventricles. So long as the heart continued to beat the pin moved, and its motions were thus recorded for some time after the cardiac sounds had ceased to be audible :—

Experiment 1.—A full-grown healthy dog was suddenly deprived

of air by plugging the tube placed in the trachea in the manner already described. Its first struggle occurred in twenty-five seconds; its first respiratory effort was not recorded, its last took place at four minutes forty seconds, and its last heart's beat at six minutes forty seconds, or exactly two minutes after the last respiratory effort.

[Eight similar experiments were performed on dogs, rabbits, and one cat. In three of these the plug was withdrawn at about four-and-a-half minutes, but the animals were found to be dead.]

From nine experiments it is seen, that in the dog the average duration of the respiratory movements after the animal had been deprived of air, is four minutes five seconds, the extremes being three minutes thirty seconds and four minutes forty seconds. The average duration of the heart's action on the other hand is seven minutes eleven seconds, the extremes being six minutes forty seconds and seven minutes forty-five seconds. Lastly, these experiments lead to the belief that on an average the heart's action continues for three minutes fifteen seconds after the animal has ceased to make respiratory efforts. The extremes being two and four minutes respectively.

In the case of the three rabbits experimented upon, we find that on an average they ceased to make respiratory efforts in three minutes twenty-five seconds; that their heart's action stopped in seven minutes ten seconds; and, consequently, that the interval between the last respiratory effort and the cessation of the heart's action was three minutes forty-five seconds.

[Then follows a series of experiments in which the plug was withdrawn at different intervals, and the dogs delivered from the pains of death, and were subsequently operated on again. The Committee say :—]

These results lead to the conclusion 1st, that a dog may be deprived of air during a period of three minutes fifty seconds, and afterwards recover without the application of artificial means; and 2ndly, that a dog is not likely to recover if left to itself after having been deprived of air during a period of four minutes ten seconds.

For some time after the occlusion of the tube in the trachea the force of the respiratory efforts was so remarkable that it was determined to adopt some means of measuring it. . . .

The following were the results :—

Experiment 15.—A medium-sized dog was treated in the above way. The respiratory efforts commenced at two minutes five seconds. As apnoea advanced they became more powerful, and from three minutes twenty seconds and onwards they were very violent till four minutes forty-five seconds, when they ceased. The needle showed the heart to be moving up to eight minutes. This dog drew the mercury up the tube, by its violent inspiratory efforts, a height of four inches, and that height was attained in almost the last attempts at respiration, four minutes forty-five seconds after the establishment of the apnoea. On examination the lungs were found

to be congested, but there were no ecchymosed spots or blood in the tubes.

[Then follow similar experiments which produced similar congestion of the lungs.]

In the following experiments the great force of the inspiratory efforts was demonstrated in another way.

Experiment 18.—A guinea-pig was held so that its nose was immersed in mercury, the animal being upside down, and the nose inserted sufficiently deep in the mercury to prevent the possibility of getting any air. The respiratory efforts commenced at thirty-five seconds, and ceased at one minute thirty-seven seconds. On examining the lungs they were found full of globules of mercury, which had thus been drawn up by this weak animal a distance of an inch or two, and that in spite of gravitation.

Experiment 19.—A terrier was deprived of air by plunging its head into liquid plaster of Paris, the object being, to see, through the whiteness of the plaster, whether any of the fluid obtained access to the lungs. Respiratory efforts commenced at one minute thirty-five seconds, and ceased at four minutes, the heart beating till five minutes. On examining the lungs the white plaster was found throughout the bronchial tubes.

From these preliminary experiments on the effects produced by simply depriving an animal of air, the Committee passed on to the consideration of drowning. The first point to be ascertained was, "for what period can an animal be submerged and yet recover without the aid of artificial means?"

Experiment 20.—A medium-sized dog was fastened to a board and submerged in a large bath. It was removed in four minutes, but although the heart went on acting for four minutes and a half longer, it neither gasped nor moved.

[Two similar experiments follow.]

It having been thus ascertained that four minutes' drowning kills, it was determined gradually to shorten the time, in order to find out what was the limit of time at which immersion proved fatal.

Experiment 23.—A dog was bound as before to the board and immersed for three minutes fifteen seconds. On being taken out of the water no respiratory efforts were made; the dog was dead. Bloody froth escaped from its mouth, and its lungs were full of the same material.

Experiment 24.—The same as above, but the dog's head was kept under water two minutes only. The dog gasped once or twice and then died. Lungs full of blood and watery froth,

[Five other dogs were then submerged and removed at graduated intervals of time.]

Having seen that a dog lives after being submerged one minute, and dies after being submerged one minute thirty seconds, another experiment was performed.

Experiment 30.—A large dog was submerged one minute fifteen seconds. On being removed it perfectly and almost immediately recovered.

Thus then the remarkable fact appeared that, whereas in simple apnoea recovery may be possible after the deprivation of air for three minutes fifty seconds (Experiment 13), and subsequent experiments showed that a dog simply deprived of air almost certainly recovered after four minutes, one minute and a half's immersion in water suffices to destroy life.

Now, to what is this striking difference due? With reference to this question the following experiments were performed :—

It was resolved in the first place to eliminate the element of exhaustion produced by struggling; it was thought that possibly the violent struggles of the animal to gain breath, when its limbs were confined, might exhaust it and hasten the catastrophe.

Experiment 31.—A cat was placed in a cage, and the cage plunged under water. The animal's limbs were at perfect liberty, and there were no violent struggles. After two minutes the cage with the cat in it was taken out, and the cat was dead.

Experiment 32.—A dog was treated in the same way, but the cage was kept submerged in the water only one minute and a half. The dog died. There had been no struggle.

Thus it was seen that struggling had nothing to do with the early fatal result, as it happened equally soon where there were no struggles.

It was next determined to eliminate the element of cold, and for that purpose the following five experiments were performed, in which cold was applied to no part of the surface except the animal's head.

[All the dogs died.]

Still further to clear up this question it was determined to place two dogs under precisely similar circumstances, with the single exception that in the one case the free access of water to the lungs should be permitted, and in the other case prevented. The following were the experiments :—

Experiment 38.—Two dogs of the same size were fastened to the same plank and submerged at the same moment, but one of them had previously had its windpipe plugged in the usual way, and the other had not. At two minutes they were taken out together; the one that had been plugged at once recovered, the other died.

[This was repeated, and the committee add :—]

These experiments satisfactorily show that the difference between

apnoea produced by plugging and that by drowning is not due to submersion, to depression of temperature, or to struggles.

The fact that animals do not recover after so short a period of immersion is mainly due to the entrance of water.

[Three other experiments were tried with windpipes plugged, after which chloroform was used.]

Experiment 43.—A medium-sized dog was rendered insensible by chloroform and drowned. It was kept in the water for two minutes and a half. Its respiratory efforts were by no means violent, and were in this respect in strong contrast with those of the unchloroformed dogs.

[Two other experiments of a similar kind followed, which proved:—]

That by simply depriving the animal of the power of making violent respiratory efforts the period during which submersion may be continued, and yet recovery follow, is at once prolonged. The value and conclusiveness of these chloroform experiments, as showing the essential connection between the early fatal result in drowned animals and the violent inspiratory efforts that fill the lungs with water, need not be pointed out.

Various means of resuscitation were employed in many of the experiments performed by the committee, and with variable results.

[Then follow seven experiments, in all of which, save one, the animals died.]

Experiment 51.—The same dog as had been previously used in Experiment 13 was again deprived of air as before, and on the use of Dr. Marcet's instrument, one minute after respiration had ceased, it again recovered.

[This poor dog therefore recovered from death twice. The committee then say:—]

No definite conclusion concerning the relative value of the various methods of artificial respiration can be drawn from these experiments.

For this subject the committee would refer to the "Report of the Experiments upon the Dead Human Body."

Many other methods of resuscitation which have been recommended were practised, including actual cautery, venesection, cold splash, alternate application of hot and cold water, galvanism, and puncture of the diaphragm.

[Twenty-six experiments under this head were tried, in which the animals were "suffocated in the usual way by plugging their windpipes;" and the committee add:—]

Although some of the above means were occasionally of manifest advantage, no one was of such unequivocal efficacy as to warrant the committee in specially recommending its adoption.—*Report of the Royal Humane Society*, 1865, pp. 31-66.

[Numerous experiments were also made on the bodies of deceased human beings which appear to have yielded excellent results.

Reference to the Reports of the Royal Humane Society will show that with immaterial modifications the method originally introduced to the Society by Dr. Silvester is still in use; such modifications having arisen out of experiments on dead human bodies alluded to, and were not derived from the foregoing painful experiments on animals.]

On the occasion of presenting the above Report to the Royal Medical and Chirurgical Society, the Chairman of the investigating Committee, Dr. Williams, closed his own summary of proceedings by saying:—"So far then as these experiments go, they show a great superiority of Dr. Silvester's over Dr. Marshall Hall's "ready method."

Dr. Edward Smith remarked upon the great importance of the present discussion from the interest of the subject, and the fact this being the first occasion on which the Society had appointed a Committee to make scientific investigation, it might be a precedent for future action. He thought it most desirable that the Society should endeavour to accurately estimate the true value of the results which such Committees could produce. On the present occasion they had a Committee amongst whom were men of world-wide reputation, and a subject of inquiry of the highest interest, and not of greater complexity than would be found in all practical questions in medicine. The Report must be regarded in two aspects: one that of the scientific facts which had been elicited; the other in their exact application to the purposes for which the Committee was appointed—to determine the best methods of restoring the drowned. As to the facts, no one could doubt their extent and interest, the care with which they had been ascertained, and the pains taken to estimate the influence of disturbing causes.

It was in reference to the practical object in the appointment of the Committee that the Report failed. The Committee had not proved that any one of their inquiries was applicable to the drowned human subject. The time during which a man could be immersed in water and recover could not be proved by experiments on dogs, and the Committee themselves had shown that all their plans for the restoration of drowned dogs had failed. The Committee had in one part of the Report disclaimed any intention to say how far the Silvester method was fitted for the restoration of the drowned; and yet in their recommendation they advised the use of this method almost exclusively, without having in any experiment tried it, under these conditions. The recommendation to place the body prone and allow fluid to run out of the mouth, was an old recommendation; but they had inferred and not proved its value, and that only from experiments on drowned dogs which they could not resuscitate. The

experiment on dogs had shown that neither hot nor cold water alone had any value as restorative agents, but that the alternative of the two was somewhat useful; but this alternative had not been recommended for man.

Dr. Webster said that he thought the Silvester method was the best, and that the recommendation was very important. He was sorry to hear that the lives of so many dogs had been sacrificed in the experiments. He hoped that in future, if possible, experiments on living animals would be avoided.

Mr. Charles Hunter said that as he was one of those gentlemen who six years ago conducted the experiments upon the dead body for Dr. Marshall Hall, and upon those experiments the "ready method" was established, he felt called upon for a few words in its defence. He regretted that the Committee thought fit to condemn it, and observed that if the Marshall Hall method after all was a failure, the long series of experiments carefully made by him (Mr. Hunter) with others must go for nothing; and yet the original experiments were much more numerous than those made by this Committee, and perfectly conclusive in their general results to those who made and saw them.

Dr. Williams would remark in reply to some objections made by Dr. Webster as to the destruction of animal life involved in these experiments that no one experiment had been undertaken without a definite and useful object; that animal sufferings and life had been spared as much as was possible in pursuing the inquiry; and he did not think that, when it was considered how animal life was hourly and unsparingly sacrificed for the gratification of appetite, there could be any objection to the dedication of a few lives to the elucidation of a subject of real importance to the interests of humanity.—*Lancet*, No. 2,028 p. 39-40.

(78.)—[Other similar experiments were made by Dr. Waters, and the results communicated by Dr. Sharpey to the Royal Medical and Chirurgical Society, May 14th, 1861.]

The subjects of experiments were dogs, cats, and rabbits. They were drowned in water varying in temperature from 40° to 50° Fahrenheit, and in one instance 56°. On being removed from the water, after every external symptom of life had disappeared, they were opened by the removal of the anterior part of the chest, so that the movement of the heart could be observed. . . . Thirteen experiments were performed, twelve on rabbits, one on a cat. Of the thirteen seven were put into the hot bath; of these six died at periods varying from two to twenty hours after submersion. Six animals were left to themselves; of these four recovered and two died, both between the eighth and twentieth hour after submersion.

The author believes that the best method of performing artificial

respiration we are not acquainted with, is that recommended by Dr. Marshall Hall.

Dr. Babington was not sure whether experiments upon dogs, respecting warm baths, were applicable to human beings. The warm bath would probably be more injurious to asphyxiated dogs, from the fact that the skin of the dog was remarkably thick, and it was known that he did not perspire. He (Dr. Babington) did not know whether cats were subject to perspiration.

Dr. Waters in reply said that of the experiments he had detailed some were performed about four years ago. The attention of the profession was at that period directed to the subject by the late Dr. Marshall Hall. He (the author) at that time brought the results of his experiments in reference to the hot bath before the Committee of the Liverpool Royal Humane Society. This society had previously adopted the rules of the Royal Humane Society of London. The Committee of the Liverpool Royal Humane Society referred his plan to the Liverpool Medical Society for their opinion. The result was that the plan was recommended, and it was therefore adopted. Unfortunately no record whatever had been kept of the cases thus treated, so that no practical test of the working of the method was attainable. . . . The experiments of Mr. Erichsen, and those performed by himself, tended to prove that the heart contracted for some time after complete asphyxia.—*Dr. Waters, Lancet*, No. 1,969, pp. 513-14.

[After all these valuable (!) experiments the learned Dr. comes to the following honest, sensible, conclusion.]

In considering the question of the deviation of the heart's beat in asphyxia, and the possibility of restoring animation in the affection, it is very desirable that if we err we should err on the right side. It is better that we should make fifty ineffectual attempts to save life, acting on the supposition of the prolonged duration of the heart's beat, than that we should suffer one life to be lost by allowing the opposite assumption to paralyse our efforts.—*Dr. Waters, Lancet*, No. 1,977 p. 60.

(b.)

EVIDENCE OF PROLONGED PAIN.

(79.)—[Some of the following show pain only for a short time, but they could not well be eliminated from the series in which they appear.]

This pamphlet contains the results obtained by burning and scalding about thirty dogs, in regard to—1st, the local temperature

produced by burning certain substances upon the surface of the body; 2nd, the manner in which this local increase of temperature extends over neighbouring parts, and the mode in which this increased temperature dies off; 3rd, the histological alterations produced locally and generally by burns and scalds. Medium-sized dogs alone were made use of. Some of these were narcotised by the injection of half a drachm of tincture of opium into the crural vein, and others by chloroform inhalations. The latter method was employed when the blood was to be examined, the former in other cases. The burns were produced by spunging the chest and bellies of the dogs with oil of turpentine five or ten times in quick succession setting fire to it each time, the scalds by pouring over similar parts eight ounces of boiling water nine times in quick succession.

The results obtained were:—1st, all the dogs died either in a few hours or at the latest after five days.

2nd.—Excision of a portion of skin corresponding in position and extent to that burned, had no injurious effect on three dogs on which it was performed. For the first few days the wound was covered with sponge, no attention was subsequently paid them, and the wounds healed most kindly.

3rd.—In three cases the burned portion of skin was excised two, five, and fourteen hours after the burning. All the three dogs died twenty-four hours after the burning.—*Edinburgh Medical Journal*, 1868-9, p. 1,026.

(80.)—To a third I gave 193 drops of spirits of wine within the hour, but warded off immediate death by means of strychnine. The animal died in six weeks. On making the *post-mortem* examination I found slight congestion of the dura mater, but severe congestion of the liver, which was black, and literally rotten. The intestine was also severely congested, doubtless in a great measure from the action of the strychnine. In this case a large amount of alcohol was kept in the blood of the rabbit by artificial means (equal to seven to eight pints in an ordinary sized man). The result was, that there was not oxygen available to the extent required for its rapid combustion and elimination; hence the severe mischief set up by its presence.—*Mr. Lucas, British Medical Journal*, No. 724, p. 612.

(81.)—Good results are yielded more easily by a feverish than by a healthy animal. For these experiments strong guinea-pigs, rabbits, or dogs of the same origin and of the same quality have been used. Under their skin some cubic centimeter of ichor or putrifying blood was injected. After thus proceeding, the temperature of the animal rises several degrees, and all the symptoms appear which are to be observed in human beings suffering from putrid fever. If the quality of the poisonous substance be right the animal expires in a few days.—*Nature*, No. 216, p. 132.

(82.)—The author (Dr. Moritzs Roth Greipwold) could not succeed in producing ulcers by tying or occluding vessels, but in a number of rabbits he produced ulcers, closely resembling the ordinary perforating ulcers, by administering a small fragment of lunar caustic, the ulcers were mostly along the lesser curvature and the back wall. When examined within a few days of the administration of the dose the surrounding mucous membrane was found swollen and inflamed. —*Edinburgh Medical Journal*, Vol. 18, p. 951.

(83.)—Physiologists have been accustomed to examine the action of the heart in cold-blooded animals to determine the laws that regulate its movements. Dr. Hope performed a number of experiments on frogs and on turtles, to perceive the manner in which the auricle and ventricle contract and dilate; and Professor Muller, of Berlin, from the observations he made on the movement of the heart in frogs, states a general law respecting the rhythm of the heart. Cæsterreicher placed a body on the heart of a frog heavy enough to press it flat, but sufficiently small to allow the heart to be observed, and it was seen that the body was lifted during the contraction of the heart, but that during its extension it remained flat. . . . And Dr. John Reid endorses the opinion as a legitimate deduction applicable to warm-blooded animals, and physiologists still continue to pursue their investigations in the action of the heart in cold-blooded animals.

Page 412.—We consider then that we have proceeded on sound physiological principles in the series of experiments that we instituted on the denuded heart of turtles during the highest temperature of the season, to determine the cause of the sound of the heart; and we are decidedly of opinion that these animals afford a much better field for investigating the action of the heart and arriving at a correct knowledge of the cause of the sounds than is obtained from the denuded heart of warm-blooded animals. In the warm-blooded as in the dog and ass, the operation of laying open the thorax and denuding the heart produces a great shock on the system, and the fact that you require to maintain artificial respiration to continue your investigation for any length of time interferes materially with the action of the heart, rendering it weak and irregular, and the sounds indistinctly heard. But in turtles the effect of the operation on the action of the denuded heart appears but slight.

If the temperature of the body be high the heart continues to pulsate with energy and in a normal and regular manner after being exposed, and the animal will survive for several days, affording an ample field to observe true action and investigate the cause of the sounds.—*Dr. Paton, Edinburgh Medical Journal*, 1873-4, pp. 407-12.

(84.)—Who would have supposed that a rat's tail after removal of

the skin might be kept in a glass tube for sixty-two hours at $15-17^{\circ}$ Fahrenheit; or kept for a still longer period in moist air at 121° Fahrenheit; or after being subjected to a temperature 31° below the freezing point; or, finally after being dried in an air-pump over sulphuric acid, and enclosed in a glass tube for three days, then exposed to a temperature of 175° Fahrenheit in a hot air chamber, and again enclosed in a glass tube for four days—who would have supposed that the unfortunate tail might be subjected to such a treatment and yet live on its being placed below the skin of the back of a rat. These remarkable facts regarding the vitality of the tissues have been ascertained.

I had an opportunity of witnessing the results of two such experiments. [Amputating tails of animals and afterwards engrafting them on other animals.] In one the revived tail had been frozen; the other it had been kept in moist air for three days at 121° Fahrenheit; on the animals being injected it was found that there was free vascular communication between the engrafted tail and the surrounding tissues. He moreover finds that the tissues which have been subjected to such modifying influences are liable to fall into certain diseased conditions, the progress of which may be traced by killing the animal at different stages. In the prosecution of this research he is still engaged. He has also succeeded in joining together animals, not only of the same, but of different species, not only rats to rats, but actually a rat to a cat. He effected this by denuding corresponding parts of their sides, and then uniting by means of sutures the skin of the one animal to that of the other, and tying the two animals together so as to prevent their tearing themselves apart. The practical importance of such researches does not require to be dwelt upon.—*Dr. Rutherford, Journal of Anatomy and Physiology*, 1867, p. 163.

(85.)—His investigations have extended over many years, and he has experimented upon cats, rabbits, crows, fowls, pigeons, and various small birds. Of all these he finds young cats the best adapted for research, as the nerve trunks in those animals contain very little connective tissue in their interior. The mode of operating consisted either in dividing the nerve with the knife, or in applying a fine silk ligature for a few moments, which is almost equally efficacious in cutting through the axis cylinders. The changes induced were examined at various intervals of time ranging from two hours to six months, and both in the fresh state and after maceration in different preserving fluids.—*Lancet*, No. 2,560, p. 421.

(86.)—Vulpian removed completely this ganglion in dogs, on the left side. At the end of from ten to fifteen days the animals were

curarised and artificial respiration kept up. The skin and subjacent tissues of different parts of the body were stimulated by strong induced currents. Each time the pupil on the left side dilated a little from the quarter to a third of its radius.—*Journal of Anatomy and Physiology*, 1873, p. 398. —

(87.)—After extirpating the superior cervical ganglion in a dog it was curarised after the lapse of from ten to fifteen days. Artificial respiration was kept up and the skin of the abdomen and hinder extremities were stimulated with strong induced currents, each time the pupils became dilated, even that corresponding to the side operated on, and which is innervated from the upper cervical ganglion (a part of the sympathetic was destroyed at the time, of extirpation).—*Journal of Anatomy and Physiology*, November 1874, p. 214. —

(88.)—A. Billder excised a piece (1.5 c. m. in length) of the left cervical sympathetic from a half-grown rabbit.

In about a month the rabbit had grown just as the other rabbits, and appeared quite sound. The left pupil was only half as large as the right, and the left eyeball projected much less from the orbital cavity than the right. The left ear was distinctly broader and larger than the right and was more hyperæmic and warmer.

In a fortnight later the difference in size was more striking.—*Journal of Anatomy and Physiology*, November 1874, p. 214. —

(89.)—Dr. Hjalmar Heilberg's (of Christiana) paper on the regeneration of the corneal epithelium, which leads off the serial, contains several points.

His mode of procedure was to scratch the surface of the cornea with a cataract needle in animals (frogs, birds, rats), and after the lapse of from eighteen to forty hours to remove the eye.—*Lancet*, No. 2,501, p. 197. —

(90.)—If one continues to administer a daily dose of alcohol sufficient to bring on intoxication one remarks in the dog from about the fifteenth a nervous excitability of quite peculiar character. The animal is melancholy and uneasy; he listens, the least noise makes him start; when the door is opened, seized with fright, he runs and crouches in the darkest corner of the room; he no longer responds when patted, he runs away and tries to bite when one attempts to take hold of him, and utters sharp cries at the mere threat of blows. This irritable and timid condition increases each day, and from the end of the first month, illusions and hallucinations becoming added to it, it is transformed into a veritable delirium. In the middle of the night

he utters plaintive moans, or even whilst all is quiet he begins to bark, the cries becoming louder and more frequent as if an enemy were approaching; speaking or calling does not re-assure him, one must interfere with a light. At last, during the day he growls without cause; then, thinking that he is pursued, he cries out, runs scared hither and thither, with his head turned back and snapping in the air.—*Dr. Magnan, Lancet*, No. 2,664, p. 411.

(91.)—What we see in the dog, in some cases, after intravenous, subcutaneous, or stomachal injections of essence of absinthe is as follows:—In the interval between the two epileptic attacks, and sometimes before the convulsive symptoms, or even without convulsions, the animal is seized with an attack of delirium. All of a sudden he erects himself on his paws, his hair bristles, the look becomes wild, the eyes injected and brilliant, staring at some particular spot where there is nothing apparent to draw his attention; he barks furiously, advances and retires as before an enemy, with open mouth he throws his head suddenly forwards, and immediately shuts his jaws and shakes them from side to side, as if he wished to tear his prey in pieces. This attack of delirium may recur several times; then the effects pass off and the animal becomes quite calm.—*Dr. Magnan, Lancet*, No. 2,664, p. 411.

(92.)—At a recent séance of the Académie des Sciences, however, Mr. Ranvier adduced certain experiments, which, if they do not absolutely disprove the ordinarily received views, at least are strongly suggestive of the suspicion with which we should regard all traditional dogmas, however high the authority by which they are supported.

The views above mentioned seem to date from the experiments made by our countryman Richard Lower, who in his "Essay on the Heart, and on the Colour and Movement of the Blood," first showed that tying the vena cava was followed by ascites and ligature of the jugular veins of œdema of the head, with copious flow of saliva and tears, resembling, as he says, the salivation produced by mercury, terminating in two days in suffocation.

Mr. Ranvier however, appears to have been dissatisfied with the accepted views on the subject, and proceeded to repeat the second experiment of Lower. He tied the two jugular veins at the inferior part of the neck in a dog and in a rabbit. To his surprise, however, these animals presented no discharge of tears, no salivation, nor any œdema of the head. In other experiments he ligatured the femoral vein immediately below the crural ring in the dog; but here again no œdema occurred either on the day of operation or at any subsequent period. These results consequently were in accordance with those observed by Hodgson in man. Lastly, he applied the ligature to the inferior vena cava, but still no œdema occurred. He then con-

ceived the idea of favouring the production of dropsy by paralysing the vaso motor nerves, and recalling the experiments and observations of M. Claude Bernard he divided the sciatic nerve on one side in a dog whose vena cava inferior had previously been tied. On this side a considerable degree of œdema immediately supervened, whilst the opposite hind limb remained in its ordinary condition.

This remarkable experiment was performed three times, and on each occasion with the same results. From these experiments M. Ravvier believes that he is justified in concluding that mere ligature of the veins does not in the dog at least produce œdema, but that after obliteration of the veins, dropsy may be caused by section of the vaso-motor nerves.—*Lancet*, No. 2,428, p. 383.

(93.)—Dr. Tschaussow has lately made some observation of interest on the inflammatory process as observed after ligature of the artery supplying the inflamed part, and his results are recorded in a recent number of the *Centralblatt*. He remarks that, under ordinary circumstances, the several stages of an acute inflammation proceed with such rapidity that it is difficult, if not impossible, to define their limits. It was therefore suggested by Dr. Samuel that means should be adopted by which the progress of the inflammation might be retarded, and to this end it was practicable either to apply cold or to ligature the artery supplying the part with blood. These suggestions were carried out by Dr. Samuel himself to a certain extent. The part investigated was, for the sake of convenience, the ear of the rabbit, and in this, inflammation was established, after ligature of the common carotid or auricular arteries, by the rubbing in of croton oil. Dr. Samuel found that in the first stage (within twenty-four hours) of acute inflammation there first occurred retardation, and subsequently arrest, of the venous circulation: white “vesicle” appeared in the veins. Then followed a second stage, characterised by congestion of the arteries, which previously presented no visible change whatever. This was followed by intense arterial congestion with exudation, cloudiness, and swelling of the tissue. By means of this secondary congestion the arrest of the circulation was overcome, or, if not overcome, the death of the part was the result.—*Lancet*, No. 2,416, p. 849.

(94.)—Ablation of the cerebellum is therefore equivalent, for a time, to extensive irritation of the organ, but this gradually subsides, and in those that long survive the effects of the operation may wholly disappear. The only permanent change Mr. Mitchell thinks he has seen is, that in all the birds there is an incapability of prolonged exertion, the animals tiring much sooner than their uninjured fellows, but beyond this, no locomotor defect, no alteration either in sensibility or in the sphere of emotional activities is perceptible. Vomitting

is an occasional but transitory symptom. Diarrhoea commonly follows ablation, and persists for a week or more.

After having concluded these investigations, Dr. Mitchell examined the effects of freezing the various parts of the nervous system, and found that complete refrigeration of the cervical region of birds induced asphyxia by paralysis of the respiratory nerves, but mere chilling produced gasping respiration, convulsions, backward movements and other symptoms which strongly reminded him of the effects of cerebellar lesions. Freezing the cerebellum induced precisely similar symptoms, and this he attributes to the congestion which cold secondarily occasions acting as an irritant along the plane of junction of the frozen with the unaffected parts, and this view is supported by the fact that direct irritation of the cervical spinal cord with capsicum occasions the backward movements after some hours, the effect being also rather persistent. Dr. Mitchell made the remarkable discovery, that cold applied suddenly to definite tracts of skins in pigeons gives rise to exactly the same retrogressive movements as when the correspondent spinal regions are frozen, and when the right or left side of the crop was frozen, the pigeon walked to the side opposite to that frozen. But a still more curious point remains to be noticed, namely, that in animals from whom the cerebellum had been removed the effects of congelation and of irritants were still the same, the birds being yet capable of exhibiting in perfection the retrogressive acts, convulsions, and lateral walking.—*Lancet*, 1869, No. 2,393, p. 57.

(95.)—In a memoir recently published by Herr Dr. Zalesky, this physiologist records some experiments which seem to prove that urea is found in the kidneys, and not in the blood. He removed the kidneys from various animals, and examined the blood subsequently for urea, but without finding larger traces than usual. In animals, however, in which (without removing the kidneys) he placed a ligature upon the ureters, he found the proportion of urea in the blood largely increased, as the consequence of several experiments upon birds, reptiles, and mammals.—*Lancet*, No. 2,218, p. 239.

(96.)—Experiment 6.—A very large and powerful dog, in admirable condition, was subject to a rigid fast for seventy-two hours, three full days.—*Dr Harley, Lancet*, No. 1,938, p. 388.

(97.)—Dr. Harley briefly related several of the experiments he had performed, some of which lasted twenty-five minutes, while others had extended over a period of eighty days. In the first case, one grain of common arsenic was injected into the jugular vein of a cat ; in three minutes convulsions commenced, and in the short space of

twenty-five minutes the animal was dead. In the case referred to where the animal lived eighty days the dose was gradually increased from a quarter of a grain to one grain; in all forty-nine grains of the poison were taken, and at death the animal presented in a marked degree the effects of chronic poisoning.

In reference to experiments on animals the author said that he did not pretend that poison could produce exactly the same effect as in man. There was, however, probably greater similarity than was generally supposed. He thought that as a rule the mineral poisons acted on animals in the same way as on man.—*Dr. Harley, Lancet*, No. 1,995, p. 499. [See *Langley*, No. 45; *Marcel*, *Thorewood*, and *Lancet*, No. 126; *Yeo*, No. 172; *Reynolds*, No. 175; and *Moore*, No. 183.

(98.)—I have also ascertained that in frogs rendered blind these experiments give the same results.—*Dr. Brown-Séquard, Lancet*, No. 1,818, p. 4.

(99.)—*Dr. Soutougnine* (Physician to the Cesarewitch) also injected the blood of a rabbit into the veins of a dog with the effect of causing bloody urine, wasting and death.—*Dr. Madge, British Medical Journal*, No. 680, p. 42.

(100.)—In the first eleven cases blood of various kinds of animals was transfused in all cases defibrinated. The blood of the dogs, was transfused in two cats, and in seven dogs the blood of calves was transfused, cat's blood was thrown into one dog, and dog's blood into a sheep. In two cases in dogs, death took place from twenty to twenty-two hours after transfusion of calf's blood, and the author in one case attributed the death to paralysis of the heart, whilst in the second there was oedema of the brain and lungs. In all the rest of the cases the transfusion of defibrinated blood by different species of animals was not only well borne, but it gave the animals strength, like the blood of their own species. The author found the foreign blood to be assimilated in from twenty-four to sixty hours.

In eight cases, again, the leg of a dog was amputated and again anemia was caused. In the space of from ten to thirty minutes after the amputation defibrinated blood from calves or sheep was injected. In all cases death was the result from hæmorrhage from the stump. Of two dogs which had blood transfused into them twenty-four hours after amputation, the first outlived the operation, whilst the second died on the eleventh day of acute purulent oedema and of septicæmia. In one case transfusion of defibrinated calf's blood was made two and a half days before the operation, and it got well in twelve days.—*The Doctor*, May 1st, 1874, p. 81.

(101.)—From his paper we learn that *Dr. G. R. Cutter* has informed

him of some experiments made in Arnold's laboratory, Heidelberg, on dogs, rabbits, and frogs, the retinal vessels being measured before, during, and after the administration of quinine. A gradual but marked diminution of the arteries took place. Before death they became mere threads; the veins were also diminished in diameter; the animals often became blind, and the retina was anœmic in the highest degree. Inflammatory dilation was lessened, and the wandering of the corpuscles retarded.

Chirone's experiments further show that quinine exercises a powerful influence over the heart. A frog poisoned with the drug is able to leap or jump a considerable time after the heart has been arrested in full diastole.—*The Doctor*, October 1875, pp. 189–90.

(102.)—In order to test the statement of Reitz and Oertel that true croupous inflammation can be excited by dropping ammonia into the air passages of animals, H. Meyer introduced into the air tubes of dogs and rabbits a solution containing in each 100 cubic centimetres 8.96 grammes of ammonia eight to ten drops being used for dogs, and from three to six for rabbits. He then investigated the changes found either on killing the animals, or on their spontaneous death, which took place generally within a period varying from forty-eight to seventy-two hours. From two to five hours after the injection, the mucous membrane became swollen and red, and studded with numerous ecchymoses; colourless blood corpuscles lay around the vessels. From the surface, a fine membrane could be raised, consisting essentially of epithelial cells; under it the basement membrane lay exposed. The epithelial cells had undergone very various changes of form, being partly cupshaped, with an opaque granular protoplasm, without cilia; most of them, however, were reduced to fragments. Among with them were a few pus corpuscles and a few fungous growths of the various forms. The whole was imbedded in a mass of mucus which, under the action of alcohol, formed a fine network or ran into nuclei. The trachea was filled with bloody mucus as far as the larger bronchial tubes; the mucus membrane of the larynx was pale and cedematous. After ten hours the frothy secretion was replaced by a muco-purulent one, becoming purely purulent in the deeper part; the false membrane was yellower, and consisted almost exclusively of round cells, nucleated adipose cells, free nuclei, fat nuclei, cryptogamic growths, and mucus. Under the action of hardening materials, the latter formed a delicate trellis-work, which, with its enclosed cells, closely resembled the network present in croup. The mucous membrane was enormously hypertrophied, and presented an increased infiltration of proliferating cells. The mucous membrane of the larynx was pale and remarkably swollen; the epithelium was in part fatty. If the animal died or were killed when the disease was at its highest point, the air tubes were found to be lined with a nearly tubular membrane, from half a centimetre to a millimetre in thickness.

extending nearly as far as the bifurcation, and ending here in a dirty yellow puriform mass.—*British Medical Journal*, No. 707, p. 82.

(103.)—Dr. Chrzouzewski details some experiments in the Wiener Medical Wochenschrift. The anus and urethra being covered up, and in thick haired animals the skin shaved in a two per cent. solution of muriate of morphia, the animal died in eighteen to twenty hours; in one per cent. of strychnia, in two and a half to four hours; in one per cent. of nicotine, in one to one and a half hours; in two per cent. of cyanide of potassium at 2° C., in a half to one-third of an hour; more quickly at a higher temperature.

In order to test the method of absorption such experiments as the following were instituted:—Ferro-cyanide of potassium was injected into the vein of a dog, and the animal placed in a bath containing an iron salt, in three to five hours the veins and capillaries of the skin were stained an intense blue, the cellular tissue remaining colourless.—*The Doctor*, April 1872, p. 70.

(104.)—The cause of death after the skin of animals has been covered with varnish has been the subject of much discussion. It has been ascribed to asphyxia, reduction of temperature, retention of perspiration, &c. Dr. Feinberg has repeated the experiments, and he considers that the symptoms are due to a general dilatation of the entire vessels of the body.—*The Doctor*, February 1875, p. 36.

(105.)—Dr. Proegler experimented on rabbits, by suppressing the function of the skin by painting them partly over either with oil, gum, varnish, &c., or producing inflammation of the skin by croton oil and turpentine. He experimented on thirteen rabbits. Two of them died after forty hours, four in the first twenty-four hours.—*Mr. Purdon*, *The Doctor*, February, 1872, p. 39.

(106.)—The *Lancet*, No. 2713, contains an article approving experiments made by Dr. Sokoloff on rabbits and dogs by painting their skins, after these had been denuded of hair, with substances to prevent transudation of moisture.

Various varnishes were employed, but it was found that the most satisfactory application was a thick oil. A comparison of a considerable number of experiments showed that the effect on the internal temperature varied according to the area of the skin on which the application was made. If the application was a very partial one, during the first days there was either a slight elevation of temperature (in the dog) or a slight fall (in the rabbit) but the deviation from the normal was never considerable. Subsequently, there was a slight rise, followed by a fall, and simultaneously a series of symptoms indicated the commencement of serious organic mischief, general weakness, loss of appetite, evidence of weakened heart, dyspnoea,

&c., and these symptoms continued till death.—*Lancet*, No. 2,713, p. 318.

(107.)—He has thus removed from cats and dogs, after opening the abdomen, a part of the liver by means of a galvano-caustic knife. By proceeding slowly there was no bleeding, and the animals completely recovered.

The autopsy of these animals was made three weeks after the operation. We found in a rat from which a notable part of the liver had been removed, and which had not presented any symptoms of jaundice, the liver quite healthy and in the part cut numerous and strong adhesions to the stomach and part of the intestines.

We also removed a portion of the kidney from two dogs. In one we cut the kidney through almost its entire length, opening up the pelves. It was impossible quite to obliterate these, and as the urine dropped into the peritoneum the animal died in forty-eight hours. In the other dog we made a very deep cut in the cortical part, without opening the pelvis. This dog lived ten days without presenting symptoms of gravity, on the tenth day it fell sick and died rapidly.

With other ideas, but always in order to show the innocuity of galvano-cautery we pierced right through the thorax of a guinea-pig with a large needle, and at once cauterised this wound in the lung by means of a platinum thread made red hot by the galvanic current.—*Dr. Ominus' Lectures, The Doctor*, June 1st, 1874, p. 104.

(108.)—When rabbits are starved, glycogen disappears from the liver. In such rabbits puncture of the fourth ventricle does not produce diabetes. After a few injections of cane sugar into the stomach of starved rabbits, glycogen reappears in the liver. Injections of water, albumen, or fat, have not this effect. If the fourth ventricle be punctured before the injection, no glycogen appears in the liver, and no sugar is found in the urine. Poisoning by woorara produces diabetes in starved rabbits, although puncture of the fourth ventricle does not. After poisoning by woorara, injections of sugar into the stomach do not produce glycogen in the liver, but sugar is abundantly found in the urine.—*Dr. T. L. Brunton, British Medical Journal*, No. 686, p. 222.

(109.)—Dr. Pavy, in his lectures on diabetes, mentioned that, when he divided the superior cervical ganglion of the sympathetic, he could, at will, produce diabetes in the lower animals, but that he found it impossible to keep the subjects of the operation under observation for any length of time, because they all died in a few days with pleurisy and pneumonia. The same lesion which produced the diabetes, caused also the inflammatory affections of the lung.—*Mr. Moon, British Medical Journal*, No. 691, p. 428.

(110.)—Dr. Hughes Bennett (of Edinburgh) contributed some

"New Investigations to determine the amount of the Bile secreted by the Liver, and how far this is influenced by Mercurials." He stated that although much had been written regarding the functions of the liver, and the action of mercurials upon it, very little exact information existed on the subject. Last winter a Committee was formed in Edinburgh to re-investigate the amount of bile secreted in health, and after the administration of mercurials. This Committee was composed of Professors Christison and MacLagan, Drs. Rogers, Rutherford, Gamgee, Frazer, and Professor Bennett, the Chairman and reporter. After studying all that had been previously published by authors, the Committee made further experiments on dogs, animals best suited for the purpose. The results of four series of these experiments were given as to the amount of bile secreted, with and without mercury. In each case the weight of the animal was taken, a biliary fistula formed, the amount of food taken and analysed, and the secretion of bile for twenty-four hours measured.—*Lancet*, No. 2,299, p. 375.

(III.)—Of these, the mercury report of Dr. Hughes Bennett, which had also been brought forward at Oxford, and was now again submitted to medical criticism, was perhaps the most interesting. . . . And we are informed that there was an indisputably superior weight of opinion in favour of the conclusions expressed in the report, namely, that in dogs mercury is entirely incapable of increasing the secretion of bile, and that the analogy is sufficiently good to allow of our concluding the same of its operation in man. Now, in commenting on this conclusion, and the various arguments which have been urged against it at Norwich and elsewhere, we must in the first place express our entire conviction that the experiments themselves were performed with an amount of care and labour which renders them perfectly reliable, and for which Drs. A. Gamgee and Fraser deserve the warm thanks of the profession. It seems to us established beyond a doubt that mercury never increases, and that in large doses it seriously diminishes, the flow of bile in healthy dogs. And in spite of the objections which have been made about the infeasibility of arguments drawn from experiments on dogs (objections which are chiefly put forward by persons who are not familiar with experimentation on animals as Dr. Gamgee rightly observed), we consider that the inference of a similar ineffectiveness of mercury as a cholagogue to healthy human beings is quite legitimate.—*Lancet*, No. 2,348, p. 285.

(III.)—Professor Bennett read a "Report on the Action of Mercury on the Secretion of Bile," in which he reiterated the statements made at the meeting of the British Medical Association at Oxford, viz., that in whatever doses mercury may be administered it fails to influence the secretion of the Bile; in fact it diminishes it. An

animated discussion followed, in which Dr. Crisp called in question the accuracy of the Committee's facts, because the dog was not at all like man in the general formation of his alimentary canal. In their wild state dogs are carnivorous, in domestication they become omnivorous. He agreed with Mr. Flower that the pig would be a better animal for experiment than the dog. He thought we were bound to analyse with great care the nature of these experiments, and should not hurriedly abandon our long entertained views regarding mercury.—*Lancet*, No. 2,348, p. 292.

(113.)—Professor Gross, of Philadelphia, said that whilst acknowledging the accuracy of the facts, he was yet not disposed to ignore the action of mercury. An experiment on a dog was one thing, but a careful observation on the human frame performed at the bedside was another and wholly different one.—*Lancet*, No. 2,348, p. 292. [See also No. 172.]

(114.)—I have found that, in cats, the glycogen disappears from the liver within a few days after the bile-ducts have been tied; and also that on the fifth or sixth day after ligature of these ducts, irritation of the fourth ventricle is not followed by the appearance of sugar in the urine. It would appear therefore, that within a few days after complete obstruction to the gall ducts the liver ceases to secrete the glycogen.—*Dr. Wickham Legg, British Medical Journal*, No. 698, p. 646.

(115.)—Schiff found that diabetes could be produced by division of the anterior columns of the spinal cord between the medulla and the fourth cervical vertebra. This lasted for days or weeks, in fact till the animal died.—*Dr. Brunton, British Medical Journal*, No. 680, p. 40.

(116.)—During the past winter I have made several observations upon the changes which follow ligature of the bile ducts in animals. The animals used were cats; these seem to survive the operation better than dogs. Most observers find that dogs live only five to ten days after. The way in which the ligature was applied was as follows:—The animal was first secured in a Czermak's holder, and chloroform given largely so as to secure a deep narcosis. This latter point is worthy of attention, for, unless the animal be completely under the influence of chloroform, the bowels are apt to prolapse, and interfere with the success of the operation. Cats, it is well known, require a large amount of chloroform. A cut is then made through the linea alba, from the xiphoid cartilage downwards, for about two inches. Unless the animal be very

large and fat a longer cut is unnecessary. I have never made any longer than two-and-a-half inches. Pushing aside to the left the stomach and duodenum, and raising the free edge of the liver, the bile ducts are seen coming from the liver and gall bladder. They much resemble a vein in appearance, but they will be known by their insertion into the duodenum, bifurcation, and connection with the gall bladder. A ligature is then put around the common duct and tied close to the duodenum; another is tied tightly on the duct, about half-an-inch nearer the liver, and the duct between the two ligatures divided by a pair of scissors, the vessel being held out from the portal vein for that purpose. In two of the cats the bile found its way again into the intestines; in the latter operations, therefore, I removed altogether about half-an-inch of the common duct as is done in making biliary fistulæ. The belly walls were then brought together with ordinary sutures. It is well to place these close together, as I lost three of the cats from the giving way of the sutures and consequent prolapse of the bowels.

Had I to repeat these experiments I should choose only young, not fully grown, animals, and a warm time of the year. Though the cats were kept in a warm place, and the January of this year was mild, yet three of them were found dead one morning after a slight frost in the night.

Experiment 1. January 24th, 1873.—Large tabby cat weighing eight and-a-half pounds (3,855 grm.) very fat; bile ducts tied double and cut. The cat died probably on January 26th.

Experiment 2. January 24th, 1873.—Black she-cat weighing six pounds six and three-quarter ounces (2,757 grm.); bile duct tied double but not cut. Animal pregnant. The cat died on January 26th, examined January 28th.

Experiment 3. February 3rd, 1873.—Large tabby cat weighing, immediately after operation seven pounds three-and-a-half ounces (3,265 grm.) Animal very fat; bile duct tied but not cut. February 6th.—Cat seems to be dying; it is unable to stand, but lies on its side mewing. February 7th.—Found dead at 11 a.m. in the same place where left yesterday.

Experiment 4. January 3rd, 1873.—A young cat. The common bile duct tied twice and divided. Cat found dead on morning of 7th. Cause of death prolapse of bowels.

Experiment 5. January 7th, 1873.—White cat with yellow brown markings, very old, only one canine tooth, weighing six and a quarter pounds. Bile duct tied double and cut. January 9th.—Cat very

feeble ; no suppuration of wound. Cat found dead at 10.30 a.m. on January 11.

Experiment 6. February 3rd.—A cat not fully grown ; very wild, scarcely any fat on body ; weighing immediately after the operation three pounds five ounces (1,501 grms.) Bile duct is tied double but not cut. The animal nearly died under the chloroform but recovered with artificial respiration. Cat last seen alive on February 7th ; no jaundice of conjunctiva. Found dead on morning of February 12th, and already much decomposed. Cause of death prolapse of bowels.

Experiment 7. January 6th.—A whitish cat, with grey spots, weighing five pounds and three-quarters (2,598 gm.) Bile duct tied double and cut. January 8th.—Cat feeble, belly wound suppurating. January 10th.—Less suppuration ; cat livelier. January 13th.—Cat found dead in morning, with bowels prolapsed. Examined immediately ; weight four pounds and a quater (1,917 gm.)

Experiment 8. February 12th.—A tabby cat weighing six pounds eleven and a half ounces (3,060 gm.) The bile duct tied close to the duodenum and above, so that about seven or eight millimetres were cut out. The cat was found dying at 10.30 a.m. on February 19th. Examined immediately ; weight five pounds thirteen and a half ounces (2,620 gm.)

Experiment 9. February 12th.—A very large tabby cat, grey and black, weighing seven pounds (3,175 gm.) Very little fat on body. The bile ducts tied double, and about four or five millimetres of the duct between the ligatures cut out. In opening the belly the liver was pricked ; there was free bleeding, which presently stopped of itself. The animal is said to have died on February 21st. It was examined on February 22nd, weight five pounds two and a half ounces (2,325 gm.)

Experiment 10. June 10th.—Black-and-white cat. Common duct tied double, and about five millimetres of the duct cut away. Cat found dead on morning of June 23rd. No jaundice of lips and mouth.

Experiment 11. January 9th.—Black tom-cat, weighing six pounds one and a quarter ounces (2,725 gm.) Bile duct tied double and cut. January 22nd.—The conjunctivæ are now noticed for the first time to be yellow. The cat is growing thinner every day, but has eaten very little since the operation. The wound looks well, and there is no suppuration. January 25th.—This day all the remaining stitches taken out of the wound ; it is firmly united ; weight of cat three pounds seven and a half ounces (1,040 gm.) The cat died at 10.0 p.m.

Experiment 12. January 24th.—Black cat, weighing immediately after the operation seven pounds five ounces and a half (3,348 grm.). Bile duct tied double and cut. February 3rd.—A brown-yellow tinge now visible on conjunctivæ; most of the stitches taken out of the wound; the cat eats well. February 5th.—Jaundice is increasing; wound nearly firmly united; weight six pounds ten ounces; appetite keeps good. February 7th.—Jaundice still increasing; cat is now very feeble, and when tumbled over has great difficulty in regaining its feet. February 10th.—Jaundice extremely intense. February 12th.—Cat dying; weighs four pounds seven ounces (1,995 grm.); was left alive at five o'clock in the evening. February 13th.—Found dead this morning at ten—

Experiment 13. January 11th.—A black-and-white cat; the bile ducts tied double and cut; this cat never became jaundiced; it was killed on February 6th by cutting off the head.

Experiment 14. January 7th.—A brown-yellow cat; weight four pounds fifteen ounces and three-quarters; bile duct tied double, but not cut. January 9th.—No jaundice, but cat very feeble. January 18th.—The conjunctivæ are now slightly jaundiced. January 22nd.—Yellowness of conjunctivæ still more marked; edges of wound in belly firmly grown together; all the stitches taken out to-day. February 3rd.—Yellowness growing less; belly wound quite healed. February 5th.—No yellowness can any longer be seen in the conjunctivæ; the belly wound can scarcely be made out, it is so firmly cicatrised; weight four pounds twelve ounces; fæces dark-coloured, and urine gives no green re-action with nitric acid. It was determined to kill the animal; the head was therefore cut off.

Experiment 15. June 10th.—A tortoiseshell cat, not fully grown. The left branch of the hepatis tied once only, The cat never became jaundiced; it ate immensely, and yet it lost flesh daily. It died in the night between June 27th and 28th.

Experiment 16. June 27th.—Black-and-white cat, well nourished, full grown. Bile duct tied double, and piece cut out. July 6th.—As the cat was now very weak, and seemed about to die, it was determined to make the diabetic puncture. The cat was therefore laid prone, a cut made through the skin, over the occipital protuberance, and the chisel applied immediately underneath this. After dividing the occipital bone the chisel was passed in a direction downwards and forwards, so as to cut the line made by joining the two ordinary meatus. The chisel was pushed on until it met with the basilar bone, and was then withdrawn. Operation was over at 12.30. Before the operation the cat had languidly taken a little milk; urine passed during the operation, though highly jaundiced,

gave no re-action with Trommer's test; at 2.15 urine pressed out of bladder, likewise no re-action with Trommer's test. July 4th.—Cat still alive; urine gave no re-action with Trommer's or Moore's test. July 7th.—The cat died in the night between July 5th and 6th. Examined to-day at two o'clock. Much more peritonitis than in any other of the experiments.

In all these cases it may be remarked that the jaundice of conjunctivæ was very close to declare itself. In none was it noticed until the tenth day after the operation; in one it did not show itself until the fourteenth. The cats were daily examined for this appearance. On the other hand, Frerichs asserts that a yellowish colour of the conjunctivæ could be noticed in sixty or seventy hours after the operation. Tiedemann, Gamelin, Leyden, and Golowin also found their dogs jaundiced on the second or third day. In Heinrich Mayer's experiments the jaundice seems to have come on later, about the same time as in my own; and it may be further noticed that he used cats, not dogs, upon which circumstances the difference between observers perhaps depends. In an old experiment by Jaunders the hepatic ducts of a dog were tied. Two hours after the dog was killed. The absorbents were found distended with a fluid of a bilious colour, and white paper dipped into the serum of blood taken from the hepatic vein gave a deeper tinge than from the jugular. I have repeated this experiment in the dog without success.

Although the appetite in these animals remained good in most cases, yet they wasted; in one case the weight fell from seven pounds and a quarter to four pounds and a half in nineteen days. The cats appeared to become weaker daily without any marked symptoms of disease.

The cause of death in these creatures is very obscure. Blondlot and many others attribute it to peritonitis. Blondlot gives a distinct cause. He says that the ligature eats through the bile duct; the bile is thus poured into the peritoneum.

Leyden seems to think that it is the addition of the jaundice to the peritonitis which kills the animals.

I should be far more inclined to think the cause of death to the changes which take place in the liver. The liver in these cases, as tested by iodine, contained little or no glycogen.

Of all the functions of the liver known to us the most important is the preparation of glycogen, and this seems to pass into complete abeyance soon after the ligature of the bile ducts. Glycogen is

one of the most important elements of nutrition; and it is not surprising that the animals should have wasted so rapidly when the system was deprived of it. And it is to this defect in nutrition, even while the animal was taking nourishment freely, that I am inclined to attribute the fatal end.

In [human] disease the state which most closely imitates ligation of the bile ducts is congenital obstruction of the bile ducts outside the liver. Of these cases, of which there are but few on record, there were found, in one of the more carefully examined cases, appearances which the writer calls hepatitis interstitialis; in other words, an overgrowth of the capsule of glisson. Here the same chain of events seem to take place. The change of the bile ducts into a fibrous cord influences all the connective tissue in the portal canals, and an overgrowth takes place. The nature of these changes, whether of a so-called inflammatory origin or otherwise, it is unnecessary and would be unprofitable here to discuss.—*Dr. Legg, St. Bartholomew's Hospital Reports*, Vol. ix. pp. 161-81.

(117.)—Goltz ascribes the disturbances of movement produced by section of these organs, to the loss of the feeling of equilibrium.

The first point investigated by Solucha was, how far is an abnormal position of the head able to disturb the feeling of equilibrium of the animal, and so to produce the abnormal movements? The author confirms the experiment of Lognet that mere section of the recti capitis postici majores et minores in the dog renders the movements of the animal uncertain and insecure, the dog was unsteady on its feet, moved from side to side, kept the fore-feet widely apart from each other, running was rendered difficult, &c. After five or six days the head generally assumed the normal position, and at the same time the walking became normal. In a second series of experiments the author sought to give pigeons a peculiar position of the head, without wounding important parts, a position such as occurs in section of the canals, with the beak directed upwards and the occiput towards the ground. On fixing the head to the breast in this position with a thread, the animals conducted themselves partly like those in which the horizontal as well as the vertical semicircular canals were destroyed.

On section of one horizontal canal the animal made several lateral movements of the head, beginning from the injured side, which soon ceased. In section of the corresponding canal on the opposite side, pendulum movements of the head occurred and persisted very long. The violence of the movements increased from the beginning onwards until they reached a maximum, when the animal lost its equilibrium, fell over, executed *mouvements de manège*,

&c. In a few cases the animals recovered completely, but generally after four or five days the animal was found in a corner with the peculiar position of the head above described and quite quiet, but when disturbed it resumed the pendulum, &c. ; most of the animals died in from ten to twenty days.

On section of all four canals violent movement of the head, resembling a screw motion, occurred immediately, accompanied by general swinging movement of the whole body.—*Journal of Anatomy and Physiology*, 1873, p. 400. —

(118.)—L. Perl, in his first series of experiments (on dogs), performed seldom and large bleedings (every five or seven days, three to three and a half per cent. of the body weight at each time), in a second series more frequent and smaller bleedings (every three or four days, one to one and a half per cent.) were practised. The animals endured the operative proceedings well. The wounds healed well without fever, and only in one case did embolus of the lungs occur.

Whilst the animals of the second series, on which ten as the minimum and seventeen as the maximum of bleedings were practised, remained quite cheerful and well, and when killed, from the thirty-sixth to the thirty-seventh day, showed no signs of change in the muscles of the heart, the seven dogs of the first series, on the contrary, on which five to seven bleedings were practised, became lean, lost appetite, became sad, had partial oedema of the extremities and died (6) with the phenomena of marasmus, within eleven weeks. With a single exception, all the animals dying after four weeks showed a very flabby heart, with a yellowish colour, and under the microscope the muscular fibres were found to have undergone extensive fatty degeneration.—*Journal of Anatomy and Physiology*, 1873, p. 407. —

(119.)—He ligatured the bile duct of a dog. The animal lived nineteen days, and though it continued to have a voracious appetite it emaciated visibly. The colouring matter of the bile was found four hours after the operation.—*Journal of Anatomy and Physiology*, 1873, p. 407. —

(120.)—J. W. Legg (St. Bartholomew's Hospital) operated upon cats. The animals survived the operation for varying times up to twenty days, and peritonitis when present remained local.

The cats became emaciated and died without convulsive phenomena, and only became comatose shortly before death. With regard to the cause of death the author lays stress upon the decided diminution or absence of glycogen of the liver (tested

with iodine solution).—*Journal of Anatomy and Physiology*, 1873, p. 420.

(121.)—L. Seelig experimented on rabbits which had (p. 422) been allowed to hunger; diabetes was produced by Eckhard's method. The author then convinced himself that in the starving animals, after the diabetic sugar had disappeared from the urine, or occurred only in traces, corresponding to the results of Dock (the hunger period lasted three to five days, the collected urine was evacuated by pressure after it had been collected for six hours) . . . A solution of sugar (generally 20 ccm of a 10 per cent. solution = 2 grms. sugar) was then injected into the jugular vein, in the one case into the starved animals, and in the other into the starved diabetic ones. In the former case only traces of sugar appeared in the urine when the animals had starved for five, six, and seven days, somewhat more when the hunger period was shorter.—*Journal of Anatomy and Physiology*, 1873, p. 421.

(122.)—A. Bidder operated on the superior epiphysial cartilage of young rabbits. The cartilage was either exposed and transfixed with needles or destroyed by section, growth of the bone was arrested either on one side or over the whole extent of the terminal surface according to the part irritated, and this effect was marked throughout the whole length of the bone as far as the distal epiphysis. Destruction of the cartilage on the fibular side was followed by growth of the opposite side, causing curvature of the bone with the convexity inwards.—*Journal of Anatomy and Physiology*, 1873, p. 425.

(123.)—Dr. Burdon Sanderson delivered an address on this subject relating his first experiments as to the effect of inoculating animals with pyæmic liquids. In the autumn of 1867, he injected the purulent liquid contained in the ankle joint of a patient, who had died a few hours before with metastatic abscesses, general suppurative arthritis and intense septicæmia, under the skin in a dog and two guinea-pigs. The two guinea-pigs died within fifteen and twenty days. Both had metastatic abscesses; in one the lungs were beset with minute nodules resembling miliary tubercles. The dog lived seven weeks, there were no secondary abscesses, but miliary tubercles of the liver and spleen. From one of the two guinea-pigs, two others were inoculated; one died of pyæmic subcutaneous abscesses, without visceral disease; the other lived longer, had no abscesses but tuberculous disease of the lungs. During the same winter other experiments were made, which seemed to show that, by the inoculation of pyæmic products, two sets of lesions might be produced; as an immediate

result, metastatic abscesses, accompanied by a general typhoid state, which was often fatal, as an ulterior result, either disseminated nodules, at first hard, but afterwards becoming caseous at their centres, or interstitial induration, both forms of lesion having their seat chiefly in the lungs, spleen, and liver, but also occurring in other viscera.—*The Doctor*, June, 1872, p. 132.

(124.)—Take pus—you do not want to go to decomposing vegetable or other animal fluids, you may do it with them—but take pus, and with it I could make a case of pyœmia or scepticœmia according to order, by the length of time which I kept the pus before I injected it; and I know very well, in experimenting on this subject, you may produce all degrees of the disease, and you may say that the chance of getting secondary abscesses are in direct ratio to the length of time an animal lives after it has become inoculated with the poison. When the poison is thoroughly septic, when you have that terrible substance which Dr. Burdon Sanderson has shown us how to get in the peritoneal cavity of an animal, the blood becomes so poisoned and spoilt, that it kills outright, and there is no time for the secondary effects to supervene.—*Dr. Savory, British Medical Journal*, No. 686, p. 240.

(125.)—If, for example, you take perfectly fresh pus, not putrid in the least degree, pus that contains no flocculi, perfectly limpid and pure pus, and inject it into the venous system of a dog, you will get a great rise in temperature. We get shivering and *malaise*, and, after a certain time, the dog's recovery. You may inject a considerable quantity of pus, and you may repeat the experiment again and again, and the dog may recover. That I take to be simply pyœmia. If, on the other hand, you take pus which is no longer perfectly limpid, but a little flocculent—it need not be putrescent—which contains particles of such a size that they may stick in the vessels, and, if you can inject that pus, then you get the same train of symptoms, you get abscesses, you get purulent deposits in different parts of the body, the internal viscera, and so on. In fact, you may imitate simple pyœmia and the multiple pyœmia which we get in a human subject. On the other hand, if you take putrid matter—whether vegetable or animal—for instance, if you take cabbage leaves and let them rot thoroughly in a solution, and then get this foul stinking cabbage water, if you clear away all the solid particles as far as you can, you will by these means get a train of symptoms which may be free from metastatic abscesses. You get a high temperature, you get shivering, not always, but very frequently, you get vomiting, purging, collapse, and the rapid death of the dog.—*Mr. Hulke, British Medical Journal*, No. 686, p. 238.

(126.)—In the *British Medical Journal*, No. 718 and succeeding numbers, a series of six hundred and nineteen experiments are reported, performed by Dr. Bennett, extending over a period of four years, to show :—

1. *Antagonism between Strychnia and Chloral Hydrate.*

To eighty-three rabbits and thirty-one rats were administered doses of strychnia with the usual result of horrible torture. One of these is thus described :—

Experiment 36. On the tenth day after the former experiment one-ninetieth of a grain of strychnia was again injected into the same animal under the skin over the left loin. The animal remained quiet for five minutes, when it became restless, and moved about with a staggering gait. In two minutes more it leaped from the table, fell on its side, and had several severe convulsions, with an extreme degree of opisthotonos. These convulsive attacks occurred three times, when death ensued twenty-six minutes after the administration of the poison.

2. *Antagonism between Sulphate of Atropia and Calabar Bean.*

Before commencing in this class Dr. Fraser indicated his results in the same field of experiments. Nevertheless, Dr. Bennett proceeded to operate on one hundred and twelve animals, with the following shocking results :—

Experiment 126. Male rabbit, weighing three pounds eight ounces. Three-fourths of a grain of extract of calabar bean dissolved in ten minims of water were injected under the skin of the back. In a minute and a half there were slight twitchings of the skin. In three minutes the breathing became very hurried, and the animal seemed to be very distressed. Saliva now accumulated profusely in the mouth. In two minutes more the animal rested on its abdomen and chest, and spread out its legs, which were stiff. It attempted to gain its natural position, but in vain. The pupils were now contracted from seven-twenty-fifths (their diameter before the experiment) to three-twenty-fifths of an inch. Soft diffluent fæces were passed. At the end of eighteen minutes from the time of the introduction of the poison the animal was lying on its side. The respirations were much laboured. It remained in this condition for thirteen minutes more, with severe occasional muscular tremors, when it died; that is, thirty-one minutes after receiving the three-fourths of a grain of the extract.

3. *Antagonism between Hydrate of Chloral and Calabar Bean.*

Sixty animals in this class were tortured, their expressions being "profuse salivation," "severe tremors," "asphyxia," "very ill," "much affected," "very ill for eight hours," &c.

4. *Antagonism between Sulphate of Atropia and Meconate of Morphia.*

Notwithstanding the numerous experiments and observations which have been made on this subject, a careful investigation into the evidence which existed previously to the committee's inquiry could not but demonstrate that nothing positive or certain had been arrived at.

One hundred and eighty-one rabbits and dogs were tortured as follows :—

Experiment 299. Rabbit weighing three pounds and a half. Cardiac impulses forty-five in ten seconds. Respirations twelve in ten seconds. Pupils measured in their transverse diameter twelve-fiftieths of an inch. Six grains of meconate of morphia in thirty minims of water were injected subcutaneously under the skin of the back. In three minutes the animal lay on its abdomen and chest, with the hind legs extended and stiffened. The transverse diameter of the pupil was now about eleven-fiftieths of an inch. Respirations ten in ten seconds; cardiac impulses forty-two in ten seconds. In two minutes more the animal attempted to walk, and it progressed forwards with evident difficulty, owing to the weakness of the posterior extremities. When moving a slight push was sufficient to turn the animal over on its side. In eight minutes more there were slight convulsive twitches of the muscles of the back, and the animal was now quite narcotised.

Experiment 300. Thirty minutes after receiving the dose the respirations were reduced to six in ten seconds; the cardiac impulses fell to thirty-eight in ten seconds, and the pupil now measured six-fiftieths of an inch. The muscular twitchings had increased. After a little time there were severe spasms coming on with great suddenness, accompanied by bending backwards of the spine and pawing movements of the fore limbs. These spasms continued for nearly thirty minutes.

Experiment 307. Twenty minutes after a dose a dog was much excited. It sat on its haunches and swayed its head from side to side. Occasionally it tried to walk, but there was evident weakness of all the limbs, more especially of the hind extremities. On placing the hand over the wall of the chest the pulsations of the heart could be distinctly seen and felt. The animal continued in this state for nearly four hours when it began slowly to recover. It appeared to be out of health and frequently refused food for four days after this experiment.

A week after the preceding experiment the same dog weighed fifteen pounds, and was made to take a second dose of poison.

6. *Antagonism between Tea, Coffee, Cocaine, Theine, Caffeine, and Guaranine on the one hand, and Morphia on the other.*

One hundred and seventy-six dogs, cats, rabbits, mice and frogs

were under treatment, the expressions of whose sufferings are too horrible to peruse. The following extracts are only two out of many detailed descriptions:—

Experiment 430.—Six grains of theine. Cat. Death. Post-mortem examination.—Six grains of theine, dissolved in a drachm and a half of water, were injected under the skin over the back of a healthy cat weighing four pounds one ounce. In ten minutes the animal became very irritable and angry. Fifteen minutes later this excitement had increased; the animal had a watchful anxious appearance, prowled about, and when touched with a stick bit at it and growled. If any noise or motion were made it arched the back and made a hissing noise. The legs appeared weakened, and although it could walk about it preferred sitting in a corner of the room. Its mouth and tongue were very red, and there was an abundant secretion of saliva, which constantly trickled out of its mouth. The cat defæcated and micturated several times. Forty minutes later it continued in much the same condition. Salivation was profuse. Animal suffered tenesmus, and it had a constant straining from the bowel of a clear fluid like mucus. The limbs, especially the posterior ones, were much weakened, but the animal could still run with difficulty. It could not jump; it made attempts to do so over a bench about two feet high, but failed. The breathing was laboured and irregular. The redness of the tongue and mouth, as well as the excessive irritability of the animal, had disappeared. It was quiet, lay in a corner, stupid and drowsy. It drank freely of water. Twenty minutes later it was prostrate and lay on its side, its limbs quite helpless. It paid no attention to a pinch of the toe or a blow on the tail with a stick. It seemed, however, to be intelligent, as its eyes watched every movement of the observer, and when the hands were clapped before its face it growled. The salivation and discharge from the bowel were excessive. Pupils were contracted and the breathing was laboured. Five minutes later the cat took a series of tetanic spasms and shortly afterwards died.

Experiment 433.—Twelve grains of theine. Rabbit. Spinal cord exposed during life.—A healthy white rabbit, weighing two pounds two ounces was carefully fastened down on its belly. An incision was made through the skin along the upper part of the spine, about two inches in length, and the vertebral column exposed. By means of bone forceps and scissors portions of the vertebræ were removed so as to expose a piece of the spinal cord about a quarter of an inch in length. On touching the posterior columns with the point of a blunt needle the animal struggled violently and uttered loud cries.

With regard to the dogs, Dr. Bennett says, "These experiments were considered to be so unsatisfactory that they were abandoned."

7. *The antagonism between the Extract of Calabar Bean and Strychnine.*

Twenty-four rabbits were tortured under this head.

8. *The antagonism between Bromal Hydrate and Atrophina.*

Forty rabbits were tortured under this division, Dr. Bennett closing it by saying, "The experiments were not encouraging as all the animals died." The conclusion shows torture to six hundred and nineteen animals; and this is not enough Dr. Crichton Browne says (No. 743, *British Medical Journal*, p. 409), and he proceeds therefore, to give picrotoxine with chloral hydrate to another large number of animals, which produces in them spasms in all the muscles of the body, and causes biting of the tongue, foaming at the mouth, &c., &c.

A concise answer to the question, what is poison? really seems more difficult than ever. Men gradually habituate themselves to the use of opium, tobacco, &c., till their daily dose is sufficient to kill from two to ten of their own species. Sheep have been known to consume unwholesome plants till their flesh becomes uneatable. Goats will feed on hemlock; hedgehogs swallow almost anything; and the common toad cares little for hydrocyanic acid. Ultimately we come to the acari, which appeared to enjoy a perfect immunity from the usual effects of a so-called poison; for here strychnine is only a poison in the same sense that starch would be a poison to a man, viz., in that it does not contain every element necessary for the reproduction of tissue.—*Lancet*, 2,015, p. 389.

Experiments on animals, already extensive and numerous, cannot be said to have advanced therapeutics much. I have seen Dr. Richardson give a pigeon enough opium to kill a strong man, and yet the bird was in no way affected; and I have heard of goats feeding on shag tobacco, and rabbits on belladonna leaves without taking any harm; yet from these experiments to infer that belladonna and tobacco were innocuous to man would be a grave error. Probably calomel given to a healthy dog might cause a temporary irritation and congestive obstruction of the animal's biliary apparatus, thus showing that calomel has an action over the liver; but I cannot see my way clear to infer the action of mercury on a sick man from what we see of its action on a healthy dog.—*Dr. Thoroughgood, Medical Times and Gazette*, October 5, 1872.

Dr. Marcet said if he understood rightly Dr. Harley's meaning, his interesting papers showed that small doses of arsenic continued for a long time do produce poisonous effects. A question, however, was still open to discussion, viz.: how are we to account for the reported innocuous and even protective effects from the practice

of arsenic-eating, which it is stated is carried on extensively in Styria? Indeed we are given to understand that in Styrian Arsenic Works the workman take arsenic with a view of escaping the poisonous effects of its fumes.

He (Dr. Marcet) observed finally that men and animals were not equally affected by poisons, a fact which it was important to bear in mind when experimenting with poisons on animals with the view of applying the results to mankind.—*Lancet*, No. 1,995, p. 499. [See also *Langley*, No. 45; *Harley*, No. 97; *Yeo*, No. 172; *Reynolds*, No. 175; and *Moore*, 183.]

(127.)—1st. If I divide the posterior column and almost the whole of the lateral column of the spinal cord, with the posterior and central parts of the grey matter in the dorsal region in a guinea-pig, I find, when the animal has become epileptic, that the irritation of the part of the face and neck which I have called epileptogenous determines reflex convulsive movements everywhere, except in the posterior limb on the side of the injury. This lack of reflex movements is not due to a paralysis of the nerves serving to voluntary movements, as, if that limb is at first a little paralysed after the operation, it soon recovers power, and has no trace of weakness by the time epileptic fits can be provoked.

2nd. If only the posterior column and a very slight part of the grey matter, with a still slighter part of the lateral column of the apinal cord, is divided in a guinea-pig in the dorsal region, the four limbs are attacked with reflective convulsive movements when the epileptogenous zone is irritated. In this case the encephalon communicates with the posterior limb on the side of the injury for both voluntary and reflected convulsive movements.

3rd. If in another animal the lesions mentioned in the preceding experiments have been made at the same level of the cord, one on the right side the other on the left side, I find that the two sides of the face and neck acquire the epileptogenous power, and that fits can therefore be produced by the irritation of either side, but whether the right or left side be irritated, there are reflected convulsive movements only in three limbs, the posterior one on the side where the lateral column of the cord is divided remaining without the least convulsion, while the three other limbs are violently convulsed. Both lower limbs, however, remain endowed with strong voluntary movements.

4th. In animals having had a complete section of a lateral half of the spinal cord at the level of the vertebræ, and having become epileptic, I have ascertained that the voluntary movements after a period of very great diminution in the posterior limb on the side of the lesion, return gradually almost to the normal condition in a variable

number of months. In many guinea-pigs having recovered voluntary movements, even for a year or eighteen months, I have seen but very slight convulsions in the posterior limb on the side of the injury. Re-union of nerve-fibres serving to voluntary motor fibres can therefore take place to a very great extent in the spinal cord after having been divided, but there is hardly any re-union for the nerve-fibres which in attacks of epilepsy give rise to reflected convulsive movements.—*C. E. Brown-Séquard, Lancet*, No. 2,418, p. 2.

(128.)—Fortunately animals may not have any apparent diminution of either voluntary movements or sensibility after the exposure of the spinal cord to the air.—*Idem, Lancet*, No. 1,819, p. 28.

(129.)—Before the operation in rabbits the most energetic pinching of the skin produces agitation but no shrieking; after the operation, on the contrary, the least pinching produces shrieking, and a much greater agitation. Sometimes the hyperæsthesia is so considerable that the least pressure upon the skin makes the animal shriek. Whether the operation is performed on the lumbar, the dorsal, or the cervical region the phenomena are always the same; that is there is a manifest hyperæsthesia in the various parts of the body which receive their nerves from the part of the spinal cord which is behind the section. It has been so in all the animals I have operated upon, and I have already made this experiment upon animals belonging to more than twenty species.

As long as the animals live after the section of the posterior columns, hyperæsthesia continues to exist, except in the cases where re-union takes place between the two surfaces of the section; but hyperæsthesia is greater during the first week after the operation than it is after a month or many months.—*Idem, Lancet*, No. 1,819, p. 29.

(130.)—In a mammal the spinal cord is laid bare at the level of the two or three last dorsal vertebræ, and a lateral half of this organ (including the posterior, the lateral, and the anterior columns, and all the grey matter, on one side) is divided transversely. The animal is left at rest for a little while, and then it is ascertained that sensibility seems to be much increased in the posterior limb on the side of the section.—*Idem, Lancet*, No. 1,820, p. 53.

(131.)—To obtain a very striking result from the experiment which consists in only one section of the lateral half of the spinal cord; it is better to make it after the posterior columns have been divided.

We know that after this division there is hyperæsthesia in the parts of the body which are behind the section; if, after having ascertained this fact, the section of a lateral half is completed where the posterior columns have been divided, we find that the hyperæsthesia seems to increase in the side of the second operation, while in the opposite side, not only the hyperæsthesia, but sensibility entirely disappears.

A longitudinal section is made on the cervico-brachial enlargement of the spinal cord, so as to separate it in two lateral halves—I ascertain then that sensibility is lost in the two anterior limbs, while it remains, and even seems to be increased, in the two posterior limbs.—*Idem, Lancet*, No. 1,820, p. 54.

(132.)—If the longitudinal section is more than two inches long, it is not sensitive in all its length. When there are three pairs of nerves attached to it, the one nearest to the transversal section is hardly able to give slight sensations; the next is a little more sensitive, but much less than in a normal condition; and the third is very sensitive, though not so much as the others, on the same side and behind it.—*Idem, Lancet*, No. 1,820, p. 55.

(133.)—If the section be made two inches higher in the dorsal region, there is, as in mammals, though less marked, an increased sensibility in the posterior limb on the side of the section, and a diminution of sensibility in the opposite limb. The loss of sensibility is never complete, showing that the decussation is not complete. The same results are obtained in reptiles.—*Idem, Lancet*, No. 1,820, p. 56.

(134.)—The laying bare of the spinal cord, and its free exposition to the action of the atmosphere, instead of being a cause of loss or diminution of sensibility, as it had been said, seems to be followed by a marked increase of sensibility in the parts of the body which are behind the place where the cord is exposed.

The laying bare of the spinal cord even in mammals is very rarely followed, after a number of days, by any kind of accidents (meningitis, myelites, &c.) producing a diminution of sensibility.

Deep injuries to the posterior columns of the spinal cord are always followed by a degree of hyperæsthesia greater than after the laying bare of the nervous centres, hyperæsthesia which appears in all parts of the body behind the place injured.

All the parts of the encephalon which are situated in its posterior or superior side are like the posterior columns of the spinal cord in

this respect—that a marked degree of hyperæsthesia always follows a transverse section upon any of them. If a complete transverse section is made upon any part of the rectiform bodies, sensibility becomes very much increased in every part of the trunk and limbs. Hyperæsthesia is also but at a less degree, one of the results of a transversal incision in the cerebellum, in the processus cerebelli ad testes, and in the tubercula quadrigemina. . . . Every small portion of a transverse section of the conducting zone, in a lateral half of the spinal cord, contains conductors—of sensitive impressions coming from all the points of the body, on the opposite side, which are behind the place of this small portion.—*Idem, Lancet, No. 1,823, p. 137.*

(135.)—I have found that a convulsive affection very much resembling epilepsy may be produced in animals. A few weeks after certain injuries to the spinal cord, in the dorsal or lumbar region, especially in guinea-pigs, fits appear spontaneously several times a day, or, at least, once every two or three days. But the most interesting point is, that it is possible to produce a fit when we choose, by simply pinching a part of the skin. These fits consist in clonic convulsions of almost all the muscles of the head, the trunk, and the limbs, except those muscles which are paralysed.

I have ascertained that one part only, of the skin has the power of producing the fit, and this part is that which covers the angle of the lower jaw, and extends from thence to the eye, the ear, and nearly to the shoulder. It is only the skin that has the power of generating the fit, as even the three nerves that send filaments to this part of the skin can be irritated without the occurrence of convulsions. When the spinal cord has been injured only on the right side, it is only on that side that the skin of a part of the face and neck has the power of inducing fits, et vice versa when the injury exists on the left side. If the two sides of the cord are injured, the two sides of the face can produce fits.—*Idem, Lancet, No. 1,840, p. 571.*

(136.)—If we take two living animals of the same species and decapitate them by a section passing in one of them on the nib of the calamus scriptorius and in the other on the fourth or fifth cervical vertebra and cutting also in both the principal nerves of the neck and avoiding the section of the carotids, we often find that the first one has no convulsions, or in other words no agony, while the second almost always has very violent convulsions in the four limbs and in the trunk.

More than ten years ago I found that certain animals may live for many weeks, and in more recent researches for eight months, after the extirpation of the whole medulla oblongata.

In these animals all the functions of organic life except pulmonary respiration continue without any apparent alteration, showing that these functions do not depend upon the medulla oblongata as some physiologists have thought. The persistence of life in these animals was possible on account of the cutaneous respiration, but in animals in which the skin absorbs but a small amount of oxygen, such as birds and mammals, death is said to be always rapid after the extirpation of the medulla oblongata, even when care is taken to avoid the influence of the operation upon the heart. It seems, therefore, that the medulla oblongata is an organ absolutely necessary to respiratory movements.

It is known that the only two appearances of proof that the medulla oblongata is the only centre of respiratory movements, or, in other words, the only source (direct or reflex) of these movements in the cerebro-spinal axis, are, 1st, that a transversal section of the lower part of medulla oblongata causes a sudden cessation of respiration; 2nd, that when transversal sections are made on the encephalon, from its front to its back, taking away layer after layer, it is said that it is only after the greater part of the medulla oblongata has been taken away that respiration is destroyed. As regards the first of these two assertions, we have already shown the objections against it, objections which are also very good against the second assertion. But we must say a few words more of this second assertion. When, after a series of transversal sections of the encephalon, we have reached the medulla oblongata, just above the upper roots of the par vagum, we find that respiration continues almost normal. If now we cut away the part of the medulla giving origin to this pair of nerves, we find in most cases that respiration is suddenly stopped.

In weak animals after many parts of the encephalon have been taken away, the whole of the medulla oblongata and of the pons Varolii remaining, respiration sometimes continues normal, but it suddenly stops after a small part of the pons is removed.

The stronger an animal is the more parts of its encephalon can be taken away before we can destroy respiration.

In the strongest animals death occurs in a few hours and from insufficiency of respiration after the ablation of the encephalon except the whole of the medulla oblongata, and so it often is with anencephalia monsters.

A series of experiments on pigeons has given me the following results; with the spinal cord alone respiration continues a few minutes, with the spinal cord and the part of the oblong medulla giving origin to the principal excitors of respiration, the vagi, this function continues many hours (the longest duration we have seen is thirteen hours), if there is also a great part of the base of the encephalon.

phalon left respiration continues longer but I have never seen it last more than a day and a half.

It seems indeed wonderful to see animals sometimes after a slight puncture of some part of the encephalon with the point of a needle, turn round, just like a horse in a circus, or roll over and over for hours and sometimes for days, with but short interruptions.

Parts producing turning or rolling after an injury on the right side.

Turning or rolling by the right side.

1. Anterior part of the optic thalamus (Schiff).
2. The hind parts of the crus cerebri (Schiff).
3. The tubercula quadrigemina (Flourens).
4. Posterior part of the processus cerebelli ad pontem (Magendie).
5. Place of insertion of the auditory and of the facial nerves (Brown-Séquard and Martin-Magron).
6. Neighbourhood of the insertion of the lower roots of the par vagum (Brown-Séquard).

Turning or rolling by the left side.

1. Posterior part of the optic thalamus (Schiff).
2. Some parts of the crus cerebri near the optic thalamus (Brown-Séquard).
3. Anterior and superior parts of the pons varolii.
4. Anterior part of the processus cerebelli ad pontem (Lafargue).
5. Place of insertion of the glossopharyngeal nerve (Brown-Séquard).
6. Spinal cord, near oblong medulla (Brown-Séquard).

While rotation takes place it is easy to ascertain, 1st, that it is not its production by contractions resembling those of voluntary movements which causes the rolling or the turning; 2nd, that some muscles are in a state of tonic contraction; 3rd, that the trunk and neck of the animal are bent by a spasmodic action on the side of turning if it has a circus movement, and that it is bent like a corkscrew, as much as the bones allow, in cases of rolling; 4th, that sensibility and volition may remain, and there are frequent efforts to resist the tendency to turn or roll.

And now, to add to the strangeness of the fact, in this last case the muscles remain contracted sometimes for hours, sometimes for days and weeks.—*Idem, Lancet*, No. 1,841, pp. 599—601.

(137.)—M. Flourens has found that the section of the semi-circular canals, in certain animals, is followed by a strange disorder of movements, and sometimes by a rotation (circus movement). I have ascertained that the phenomena observed in these experiments do not depend on the section of these canals, as this operation may not cause these phenomena, but that they are the result of an irritation of the auditory nerve, from the drawing upon it by the membranous semi-circular canals at the time we divide them. In frogs and in mammals the direct irritation of the auditory nerve is followed by the most interesting phenomena. It is well known that in frogs

the peripheric extremity of this nerve is enclosed in a bag containing carbonate of lime; as soon as this bag is laid bare and slightly touched, and still more if it be punctured with a needle or a bistoury, the anterior limb, on the opposite side, is thrown into a state of slight convulsion, and kept almost constantly in a spasmodic pronation, and almost at every attempt to move forwards the animal turns round on the side injured. As long as it lives (many days, or even many months) these phenomena may be observed, although not quite so marked as immediately after the injury or after the first twenty-four hours. In mammals the least puncture of the auditory nerve causes rolling, just as after the irritation of the processus cerebelli ad pontem; violent convulsions then occur in the eyes, the face, and many muscles of the neck and chest. The doctrine that the nerves of the higher senses are not endowed with general sensibility (*i. e.* are not able to cause pain) seems not to be true with regard to the acoustic nerve; at least, the signs of pain given after an irritation of this pretended nerve are often as great as those observed after an irritation of the trunk of the trigeminal nerve.—*Idem, Lancet*, No. 1,842, p. 625.

(138.)—I have now to speak of the condition of animal heat in cases of alteration of the spinal cord and the encephalon. The following conclusions may be drawn from a great many facts bearing on the subject. 1st, that usually anæsthesia is accompanied by a diminution of temperature; 2nd, that hyperæsthesia almost always co-exists with increased temperature; 3rd, that in paralysis, without either a notable hyperæsthesia or anæsthesia, the temperature is nearly normal. I must remark that the state of heat of a part is due to the amount of blood, the degree of heat of this fluid, the exposure of the part to the influence of the temperature of the surrounding medium, and the temperature of this medium.—*Idem, Lancet*, No. 1,843, p. 652.

(139.)—Professor Brown-Séquard is continuing with indefatigable labor the series of his valuable researches and experiments upon the physiological pathology of the nervous system. His communications to the Academy of Medicine have formed the most marked features of the recent sitting of this learned Society. As you will doubtless give full attention to these researches when they shall have been published *in extenso*, I shall only mention a few points thereof so as to keep *au courant* of everything that crops up here at the very moment of the event. My letters must be photographs of the passing occurrences of the day. M. Brown-Séquard's most recent researches bear upon the consequences produced by certain lesions of the corpora restiformia and the sciatic nerve in guinea-pigs. According to the illustrious experimenter, when the sciatic nerve is severed in

guinea-pigs on exciting a certain determinate part of the face which he names *le zone épileptogène* epileptiform phenomena are produced. Excitement of no other portion of the face can induce an attack of epilepsy. But in order that the phenomena may take place, it is necessary that the excitement should be produced on the side corresponding to the limb where the nerve has been divided. M. Brown-Séquard showed two guinea-pigs to the Academy and reproduced his experiments before the assembly. M. Colin, one of the members, mentioned that he had often performed the division of the sciatic nerve without being able to induce epileptiform attacks. M. Brown-Séquard's experiments on the restiform bodies are equally interesting and curious. One of the results of injury to the corpora is the production of hæmorrhage under the skin of the ear. Hitherto certain kinds of hæmorrhage had been observed as the result of injury to the nervous system, for instance hæmorrhage of the kidneys in diseases of the spinal cord and hæmorrhage of the intestinal tube in diseases of the brain. But the fact now illustrated is remarkable on account of its constant occurrence. Another result is the production of dry gangrene of the ear, which, according to the experimenter, is not the consequence of paralysis produced by the section of the restiform bodies. M. Brown-Séquard exhibited a guinea-pig showing this kind of gangrene. At the last meeting of the Academy he again adverted to the subject, and stated that the precise point of the restiform body which he excited in order to produce hæmorrhage and sphacelus was the nib of the calamus scriptorius. Some particulars he added tended to show that this situation was really the one which he excited in his experiments, for instance, the paralysis of the tongue and anæsthesia of the lips—the animal could not hold the food which was introduced into its mouth. It was a well known fact that the hypogassal nerve and fifth pair take their origin in the neighbourhood of the calamus scriptorius. M. Brown-Séquard concluded by pointing out the strict analogy which exists in a great number of cases between the phenomena determined experimentally in animals and those which are observed clinically in man. On account of the extreme interest of these experiments M. Bouillaud has requested M. Brown-Séquard to repeat his experiments before a certain number of the members, which has been assented to.—*Lancet*, No. 2,380. p. 514.

(140.)—In the course of some recent experiments on the establishment of artificial epilepsy in guinea-pigs Dr. C. Westphal has been over the same ground as that explored by M. Brown-Séquard, and fully corroborates the results at which the latter observer arrived. He adds, however, some new and interesting facts. Thus, he found that if one or two slight blows on the side of the head are given to a guinea-pig they are sufficient to bring on an epileptiform attack, after which the animal again recovers its liveliness, or it remains heavy for some time and then exhibits a kind of rotatory movement

like those shown by Schiff to occur in rabbits after lesion of the crus cerebri.

If the animal survives the blows, a similar epileptogenic zone is created as in the guinea-pig treated on M. Brown Séquard's method by lesion of the medulla oblongata at certain points or section of the sciatic; and as in these last cases, the zone is near the angle of the lower jaw. Before the zone is well established, and four weeks are usually required for this purpose, the animals betray the presence of some irritation at this part by frequently scratching it. After it is established slight punching will induce tonic and clonic spasms, though the sensibility of the skin is here diminished. The rapidity with which the zone can be established may be increased by striking the animal's head on successive days, and the excitability of the zone endures for a period varying from six weeks to six months. The condition is hereditary. M. Westphal set himself to ascertain the nature of the changes induced by the blow or blows. That the condition is not produced by any alteration in the integuments is shown by the circumstance that the fits occur when the exposed skull is struck.—*Lancet*, No. 2,528, p. 195.

(141.)—Dr. Brown Séquard has with great kindness related to me his more recent researches, and shown me the interesting little animal upon which he is now experimenting. I have therefore the pleasure of being able to speak of them *de visu*, and of adding some interesting details of the note which I sent you two weeks ago. If you remember I then mentioned that the professor's recent researches were upon some of the effects of section of the sciatic nerve and injury to the corpora restiformia in guinea-pigs. A great number of the animals were shown to me in which the sciatic nerve had been severed, and in all the *zone épileptogène* did exist. I mean one single spot by exciting which the fit was immediately produced. Pinch wherever you like independently of that situation, and though the animal will not like it, and will scream more or less, there is nothing abnormal; but as soon as you excite the particular spot it goes into a fit. In this situation, which may be easily circumscribed, extending from the ear to the eye round below the jaw, and backwards to the shoulder-blade, there seems to be an obvious impairment of nutrition, the hair is much less abundant, parasitical animals are found there, &c., and besides sensibility is considerably diminished. The fits produced are quite similar to those brought on by section of the lateral half of the spinal cord.

Dr. Brown Séquard showed me some animals in which the paw of the limb where the sciatic nerve had been divided was more or less injured. This, he observed, was not a spontaneous injury, which must be attributed, as had been thought, to the division of the nerve. The dragging of the paralysed limb produced a slight abrasion, and

as soon as there was a drop of blood, the little animal set to gnawing the insensible extremity. It is also the case with rabbits. There must be, however, a drop of blood to excite the voracity of the animals. In some of them two of the claws of the feet, the two outer ones, animated by the great sciatic nerve, which alone is severed, had fallen off, while the remaining claw and middle one, animated by the little sciatic nerve, was unimpaired. By wrapping up the limb in a bag as soon as section of the sciatic nerve is performed the experimenter prevents an injury occurring.

With regard to his other series of experiments, those in the corpora restiformia, the professor showed me a great number of guinea-pigs in which I could trace the effects of injury to that particular part of the nervous system. The ears of several guinea-pigs showed the appearance of dry gangrene. In some of the animals half of the ears had already fallen. The aspect produced by the solution of continuity along the edge of the ear is quite distinct from that of a bite. In a bite the little portion of the ear seems cleanly cut off, whereas in dry gangrene it appears to have crumbled off. I have noticed the remains of two or three hæmorrhagic clots which had formed on the ear.

To conclude, I may just mention that Dr. Brown-Séquard was much surprised when some of the French journals stated that he had neglected to note the occasional occurrence of hæmorrhage in the ears of insane people. The fact is that the very object of the illustrious experimenter in mentioning the phenomena which he observed in guinea-pigs was to show that the hæmorrhagic clot observed on the ears of mad patients, and concerning the etiology of which there is still much discussion, may be due to purely internal nervous causes, and not to external violence, his chief aim in every one of these experiments being their application to the pathology and therapeutics of the human species.—*Lancet*, No. 2,382, p. 586.

(142.)—Nothnagel employs a new method for the determination of the functions of the brain. His observations are made mostly on rabbits. An incision is made in the scalp; the skull is perforated with a needle. Through the canal thus formed in the bone a very small drop of a concentrated solution of chromic acid is injected by means of a hypodermic syringe with a very slender nozzle. The scalp wound is then united by suture, and the animal does not seem to be affected, except with regard to the functional derangement incidental to the lesion. Generally they survive the operation two or three weeks, and die from causes which Nothnagel cannot explain, no constitutional symptoms being developed.—*The Doctor*, November 1st, 1873, pp. 214 and 215.

(143.)—Professor Nothnagel, of Freiburg, contributes a paper to the last of *Virchow's Archiv.*, Band lvii. (Heft. 2, p. 184), containing

an account of a series of researches he has recently made in Heidenhaur's laboratory upon the functions of the brain. With a few exceptions his experiments were made upon rabbits placed thoroughly under the influence of woorara. He acknowledges that dogs are better subjects for experiment, whilst their more convoluted brain resembles more closely than the rabbit that of man.

On the other hand, rabbits can be obtained in any number, and they are not difficult to keep. The plan adopted by Professor Nothnagel was suggested to him by Professor Heidenhaur, and is somewhat peculiar. It consists in drilling a small hole through the cranium, through which the cannula of a subcutaneous injection syringe is inserted and plunged to a greater or less depth into the brain. A minute quantity, perhaps not amounting to more than a quarter or half an ordinary drop, of a concentrated solution of chromic acid is then injected and the effects watched. The pain must be slight, as the animal often remains passive till the sutures required to close the wound are inserted. It is also obvious that in this mode of procedure bleeding and those alterations which might result from evaporation and exposure to cold are avoided. When the acid was injected directly into the ventricles, death took place in from ten to thirty minutes, the pulse becoming very slow and great dyspnæa setting in, soon followed by increased action of the heart and convulsions.

Similar experiments upon dogs, the acid being injected into the outer extremity of the gyrus postfrontalis, were attended with analogous results.

When the animals lives for a fortnight or more they appear to recover completely.

Injection into the lenticular nucleus constantly produced the above-described deviation of the limbs. Injection into the nucleus caudatus appeared for the first few minutes to be without effect, but soon the animal began to leap forward, the leaps succeeding one another faster and faster till, after some minutes, it dropped exhausted upon its side, the limbs continuing to move rapidly.—*Lancet*, No. 2,601, p. 18.

(144) --Nothnagel has continued his researches on this subject, still using the injection of chromic acid. When only one lenticular body was operated on the results were the following:—Deviation of the leg of the opposite side (right) towards the middle line and that of the same side (left) outwards, a lateral curvature of the spine with the convexity turned towards the opposite side (right) and at the same time a moderate cypposis. The animal could, however, execute all voluntary movements.

A different state of things occurred when lotn. micleilenticular is that operated on. In twenty-six cases the author succeeded with the operation, and the results in all cases coincided.

The spinal column was sometimes straight, sometimes cynoptic, but never curved laterally. The ears were erect and never laid backwards upon the neck. Respiration and action of the heart normal. If the fore limbs were carefully extended so that the animal did not lose its equilibrium, and though the feet might be placed in a very unnatural position, as was the neck, they were not drawn back as always occurs in the normal animal. Slight pinching of the tail, which a normal animal would notice, was followed by withdrawal of the foot from the unnatural position, and the animal appeared as if it would spring, but with one spring the movement came to an end, and the animal became motionless as before. The same thing could be repeated over and over again. In some cases the animal sprang four, six, or even sixteen times. The animal sat without making any attempt at spontaneous movement, and if not disturbed till death occurred, just like an animal from which the cerebral hemisphere had been removed. Nor did the animals eat of their own accord. Most of the animals died on the second or third day; but six lived till the seventeenth day and were very emaciated.—*Stirling, Journal of Anatomy and Physiology*, November, 1874, p. 210.

(145.)—On October 25th he divided the right hypoglossal nerve in a rabbit, and took out a piece about a quarter of an inch in length. Immediately after the operation, and during the whole time that the animal was under observation, the tongue was strongly protruded to the right side. On November the 27th the rabbit was killed.—*Mr. Clarke, The Doctor*, January 1st, 1872, p. 24.

(146.)—Dr. Milne Edwards also endeavoured to substitute the carbonates of iron, manganese, and magnesia respectively for that of lime in bones by an artificial diet. This diet having reduced the pigeons subjected to experiment to utter emaciation they were killed; the bones were extremely brittle and thin, but only traces of magnesia and iron were discovered in two and none in the third. So thus the composition of bone does not appear susceptible of alteration by substitution; and Dr. Milne Edwards concludes that bone is a compound of two primordial substances, viz., phosphate of lime and osseine, the immediate principle of bone. Rather a lame conclusion this after all!—*Dr. Milne Edwards, Lancet*, No. 1,977, p. 69.

(147.)—Another, and a very droll discovery, made by the same professor (who, by the way, should be called to order by some member of the Society for the Prevention of Cruelty to Animals) is that the testicle of one frog may be engrafted into the body of another animal of the same species, so that one "froggy" may "go a-woo-ing" with the testicles of another froggy, and not at all *à ses propres*

frais; this seems a "leetle" too much: His last experiment is far more intelligible than the preceding, in part, at least. He says, "If you transplant into the abdomen (under the skin) of a female frog, a few days before the laying of her eggs, a frog's testicle, such is the attraction between this body and the eggs that ulceration of the abdominal muscles takes place, the male and female elements coming into contact, and so violently that the frog dies." This last result is, I confess, the only portion of the whole paper which I can conveniently credit.—*Prof. Mantegazza, Lancet*, No. 1,931, p. 224.

(148.)—One frog died in twelve hours, another was killed, and the third, after remaining on its back in a tetanic condition for ten days, recovered.—*Dr. Harvey, Lancet*, No. 2,053, p. 7.

(c.)

EVIDENCE OF DESIGN TO TEACH STUDENTS BY VIVISECTIONS.

[The numerous Handbooks published for Students would alone provide this evidence; but Professors have spoken out their thoughts freely, and these are reported as well as the contents of Handbooks. Foreign Handbooks are extensively used in England, but these have not been quoted from. The volume published by Drs. Burdon-Sanderson, Klein, Foster and Brunton necessarily appears in this division. Its experiments might have been classed under (a) and (b), but the tendency of its teaching would not have been seen so well by such an arrangement. Consequently a large number of experiments appear under this head. That the object of the Editor and his co-adjutors was to induce young persons to perform experiments on their own account and without adequate surveillance is manifest throughout the work, by the supply of elementary knowledge and elaborate data. Not only are the names and quantities of necessary chemicals given, but the most careful description is provided in letterpress and plates of implements for holding animals during their struggles, so that a novice may learn at home without a teacher. Besides, the Editor's preface states, that the book is "intended for beginners," and that "difficult and complicated" experiments consequently have been omitted; and that of Dr. Foster allures the student by assurances of inexpensive as well as easy manipulation; for it avers that the experiments "may for the most part be conducted on frogs, may be repeated any number of times

without difficulty or expense, and so serve usefully as a means of training students in physiological study and enquiry."

Very seldom indeed is the student told to anaesthetise, and then only during an operation. It cannot be alleged that "beginners" know when to narcotise, and when not; but if they do, then the few directions to use chloral etc. are unnecessary. No doubt should have been left on this point in a handbook designed for "beginners." Besides, where will students find cautions against the infliction of unnecessary pain, and wanton experimentation? On the contrary, the student is encouraged to repeat the torture "any number of times." These facts are significant.]

(149.)—A great change is now taking place in physiological tuition in this country—a superabundance of physiological anatomy, and an almost entire absence of experiment, are no longer the characteristic feature of our tuition.

A new system of physiological tuition is rapidly extending over the country. In the London schools, in Edinburgh, Cambridge, Manchester, and elsewhere, earnest efforts are being made to give a thoroughly practical aspect to the tuition of our science, and notwithstanding the imperfect results which must necessarily ensue in the absence of suitable endowment, we can nevertheless point to the fact that the effect of these efforts has been to awaken a love for physiological research in the mind of many a student.—*Nature*, No. 204, pp. 456-7.

(150.)—It is often the tendency of young men to think themselves competent to the investigation of recondite problems in science before they have mastered preliminaries that alone could give presumptive hope of success. In this way, and actuated by this motive, I have seen young men, not naturally cruel, urged to the practice of vivisection under the belief that they were acquitting themselves of a needful though disagreeable duty. I have seen this but rarely.—*Dr. Scoffern, Belgravia*, April, 1867, p. 221.

(151.)—For the benefit of those who have not attended this course (Dr. Rutherford's) it may be well to state that the experiments are performed by the teacher with the aid of the students. The vivisections performed are few in number and confined to the frog and rabbit. The animals are previously narcotised by opium or chloral.—*Lancet*, No. 2,471, p. 3.

(152.)—Let us convince ourselves of this. On pinching the foot

of this frog you see that it jumps away in a vigorous but at the same time sedate manner. It does not seem particularly annoyed by such an irritation. We shall see how it answers to our stimulus after it is poisoned with strychnia [a few drops of a solution of strychnia were introduced under the skin]. The reasons for this procedure were explained. After waiting for a little while the skin of the animal was merely touched, with the effect of throwing the animal into tetanic spasm. You see that a mere touch on the skin produces a result far greater than a pinch of the foot did before the animal was poisoned.

I now give the animal a large dose of the poison. We shall soon find that a touch or a pinch will fail to call forth any reflex movements. The cause of this is loss of irritability of the excito-motor cells or fibres in the cord. The irritability of the peripheral terminations of voluntary motor nerves is lowered by another poison—curare. [Some curare was injected under the skin of another frog. When the motor paralysis had supervened the skin was removed from the legs, and it was demonstrated that the paralysis was due to loss of irritability by the motor nerves.] Many other substances affect nerve irritability, others affect muscle irritability.—*Dr. Rutherford, Lancet*, No. 2,476, p. 185.

(153.)—Pflüger discovered a curious fact regarding the stimulation of a motor nerve, and the effect it produces on a muscle. He found that when he stimulated with equal force the sciatic nerve of a frog at one time near the gastrocnemius and another time at a distance from the muscle, in the latter case a more powerful impression was produced on the muscle. We shall perform the experiment.

I dissect out the sciatic nerve of a frog, sever it from the vertebral column, but leave it connected with the leg.—*Idem, Lancet*, No. 2,483, p. 438.

(154.)—I shall illustrate what I have been saying by some experiments. We have here a white rabbit. . . . I now cut down upon the cervical sympathetic nerve. Let us take that on the right side for convenience. I expose the nerve about the middle of the neck. I separate it from the superior cardiac branch of the vagus, a smaller nerve which lies close beside the sympathetic, and I put a fine silk ligature round the sympathetic, and tie it firmly round the nerve. The effect of this is to paralyse the nerve as thoroughly as if the nerve were divided. Now compare the two ears and the two pupils again. The vessels in the right ear are much larger than those in the left ear. The right ear is in consequence distinctly redder than the left. You see also that the right pupil is smaller than the left one. I shall now divide the nerve immediately below the ligatured point and then irritate the nerve just above the ligature. In an

experiment of this sort we call this part of the divided nerve its "upper" end; the end of the nerve which lies below the point of section is called the "lower" end. Watch the vessels of the right ear while I faradise (stimulate with faradic or induced electricity) the upper end of the nerve. You see that they contract, and the ear in consequence becomes blanched. We shall wait for a few seconds until the irritation passes off. Now look at the right pupil while I irritate the upper end of the nerve again. You see that it dilates enormously under the influence of the irritation. We have seen then that on paralysing the cervical sympathetic blood-vessels of the ear on that same side dilate, and the pupil on the same side contracts, and that on stimulating the upper or cranial end of the divided nerve precisely the converse takes place in both parts. We therefore conclude that this nerve contains fibres whose function is to cause the vessels of the ear to contract and the pupil to dilate, and that these fibres convey their influence up the neck. As I want to keep the experiment as simple as possible I shall not stimulate the lower end of this nerve at present. I cut down the phrenic, and I open the abdomen in order that you may see the diaphragm. You see that at intervals both sides of the diaphragm are drawn downwards. I divide the right phrenic nerve in the neck. Now look at the right side of the diaphragm. It remains quite loose and flabby when the left side is drawn down instead of being tightly drawn down with it as before. I shall now irritate the lower end of the nerve. Watch the diaphragm. You see that the right half is drawn violently downwards during the irritation, and that the flabby state returns when I stop the irritation of the nerve. We conclude that the right phrenic nerve contains motor fibres for the right half of the diaphragm, and that these influences pass through the nerve down the neck.—*Idem, Lancet, No. 2,483, p. 439.*

(155.)—Under this head some general questions regarding sensory nerves were discussed, and the mode of estimating the amount of ordinary sensibility in a part was demonstrated.

I take another frog. In this case I open the cranium and remove the brain and medulla oblongata.—*Idem, Lancet, No. 2,487, p. 565.*

(156.)—He was aware that there were some who entertained the idea that vivisection was not necessary when it had for its object the mere demonstration for educational purposes of facts already known.

Those who held this doctrine appeared to him to forget that physiology was an experimental science, and that no right conception of the subject could be obtained unless the students were shown the experiments that were necessary for the demonstration of certain facts.

Now he maintained that this definite and critical knowledge

regarding the bodily organism could not be attained unless their students were shown experiments on living animals.—*Speech of Professor Rutherford at the British Medical Association, Edinburgh, 1875. — Scotsman.*

(157.)—In recent years the teaching of physiology had made a great stride in this country. Laboratories duly appointed had been and were being organised; and the method of physiological instruction had in most instances passed from the mere prelection illustrated by diagrams to an experimental exposition of the subject. In his student days the latter element was wanting, and at this moment there was distinct danger of a return to something like that miserable mode of instruction in consequence of the fanatical clamour of a number of persons excited, it must be admitted, by one or two members of their own profession.

Physiology was an experimental science, and that no right conception of the subject could be obtained unless the student was shown the experiments that were necessary for the demonstration of certain facts.

Had not every teacher repeatedly observed the altogether different mental attitude which students assumed the moment he passed from mere description to a demonstration of phenomena? He far more forcibly arrested their attention, and far more deeply imprinted on their minds the facts he would bring home to them.

Definite and critical knowledge regarding the bodily organism could not be attained unless their students were shown experiments on living animals, and he held that those authorities who seemed to be of opinion that this method of tuition might be dispensed with, were entirely overlooking the vast importance, not only to the student himself, but to the whole race, of an experimental manner of laying the foundation of a knowledge of the institutes of medicine.

It was not necessary for a sound physiological education that their students should be shown all the experiments that were needed to demonstrate physiological truths: they probably did enough if they showed experiments on the cardinal points of physiology; and he averred that all the experiments on the higher animals that were really required for the purpose of education, could be performed with the aid of narcotics. Seeing that this was so, why should it be that some had become convinced that, in consequence of the present inflamed state of the popular mind on the subject of vivisection, the right education of medical students must be abandoned? The popular mind had been abused by inaccurate and misleading statements regarding both their motives and their actions. He main-

tained that a great and deplorable error was committed when the unreasonable clamour of the anti-vivisectionists was met in the spirit of compromise instead of the spirit of stern resistance. He believed that the unfortunate Vivisection Bill which was laid on the table of Parliament conferred a dignity on the policy of the anti-vivisectionists which, but for that bill, it would probably never have possessed. It was true that there had been a withdrawal of that singular bill, according to which they were to have been fined fifty pounds, or to have been sent to prison for two months, if they had dared to show to their students any experiments even upon a narcotised animal. But the effect of the bill was not effaced; the increased boldness which it had given to the pretensions of the anti-vivisectionists was only too evident. All that they could now hope was that the good sense of the Legislature would in the end prevail, and that it would do nothing to hamper the education of medical men.

The learned professor went on to explain, with the help of numerous diagrams, the result of a series of experiments he had made in reference to the action of certain drugs on the biliary secretion of the dog.—*Dr. Rutherford, Lancet*, No. 2,711, pp. 238-9.

(155.)—M. Claude Bernard is delivering a course of very interesting lectures at the College de France "On the Art of Experimenting."—*Professor Brown-Séquard, Lancet*, No. 2,380, p. 514.

(159.)—The last nine lectures have been more recently delivered, from 1864-70, and treat of experimental pathology. In them he points out the immense value of this kind and mode of research, the necessity for the construction of special physiological laboratories (like those of Kühne at Amsterdam, and Ludwig at Leipzig) wherever physiology is taught, in order that theory and practice may go hand in hand. Referring to the absurd scruples of the anti-vivisectionists, he points out the necessity that exists for the daily slaughter of a large number of the lower animals.—*M. Bernard, Lancet*, No. 2,535, p. 438.

(160.)—Hence a student should spend much of his time in the dissecting room, the museum, the chemical and physiological laboratory, and, above all, in the dispensary and the medical and surgical wards, and post-mortem room of a good hospital. And you will permit me to say that no university school in the present day can overlook, without committing suicide, the necessity of establishing a physiological laboratory, and including pathological anatomy in its curriculum of study.—*Dr. Ross, British Medical Journal*, No. 738, p. 233.

(161.)—The future reputation of Dr. Marshall Hall as a scientific man will rest principally upon his original researches into the functions of the nervous system, particularly into the functions of the spinal cord.

We are assembled here to-day for a common object and for a good object, not merely to do honour to the memory of Dr. Marshall Hall, but to endeavour to establish some memorial which shall be of use in stimulating and inciting the junior members of our profession—those who are gifted with talent, those who are gifted with the desire of distinguishing themselves—to spend some of those dreary years of early professional life in original investigations—*Dr. Burrows, Lancet*. No. 2,371. p. 208.

(162.)—Reference has been made to a little work on physiology lately composed by Professor Huxley for the use of young children at school, in which there are certainly some remarks which should have been expressed in more cautious and guarded language. For instance, little boys and girls are advised that “it is better for testing the presence of sensation to irritate different nerves connected with the chord rather than the cut end of the chord itself:” and how to make rabbits blush artificially is suggested as an interesting experiment: “If in a rabbit the sympathetic nerve which sends branches to the vessels of the head is cut, the ear of the rabbit at once blushes.” Considering the natural instincts of children, instruction of this kind would certainly seem dangerously suggestive.—*Saturday Review*. 1st October, 1874.

(163.)—From the tone of your excellent remarks on the incorrectness of Sir Robert Christison's statement that vivisectors might safely be trusted not to perform cruel experiments, such as would shock the feelings of the community at large, I venture to hope that you will find room for a brief account of an experiment I myself witnessed when a student at the Physiological Laboratory of the University of Edinburgh.

Woorara was administered to a number of frogs, a poison which destroys all motor power, leaving sensibility unimpaired, if not actually increased, as is well known, the poor creatures being absolutely at the so-called mercy of their tormentors. The frogs were then ripped open, and by a dexterous manipulation of the intestines the mesentery was so far exposed as to be placed under a microscope without being removed from the body of the living animal. In this condition of extreme torture the poor animals remained until fifty or sixty students had examined what was to be seen. At the very least the period of torture cannot have been less than two hours, including the necessary preparation.

Now, your readers will ask for what purpose was all this cruelty?

Simply in order to see whether in the process of inflammation any of the white corpuscles of the blood traverse the walls of the capillaries; a question of not the slightest practical importance, and the solution of which has no kind of effect upon the treatment of inflammation. Furthermore, the experiment thus conducted was not merely wicked and unimportant, but even inconclusive. The best physiologists admit that in order to witness the transudation of the white corpuscles (if such takes place at all) an examination of many hours is necessary. To determine this wholly unimportant question many of the leading physiologists, both in this country and abroad, have subjected hundreds of dogs, cats and frogs to excruciating tortures of many hours duration. But even this is little to be compared with the atrocities enjoined upon students (not for purposes of discovery, but merely in order to acquire manual dexterity, or to form a more vivid conception of established facts) in the Manuals of Practical Physiology.

Surely, when such facts as these are notorious it is too much to expect that the public will quietly stand by and leave poor animals to the mercy of their tormentors. Not even satisfied with this, some physiologists have the effrontery to demand that the torture rooms in which such enormities are perpetrated shall receive not merely the toleration but the pecuniary assistance of Government. Yours faithfully, W. B. A. SCOTT, M.D., Edin.—*Letter to the Echo*.

Dr. F. Burdon-Sanderson's Lectures delivered in the Physiological Laboratory of University College.

(164.)—In 1863 the lamented V. Bezold published his well-known researches on the nervous system of the heart. Among a number of other less important discoveries, he showed for the first time the nature and extent of the influence exercised by the brain and spinal cord on the circulation of the blood. He found that when, in a curarised rabbit or dog, the spinal cord is severed from the brain, the arterial pressure sinks very considerably, while at the same time the number and extent of the contractions of the heart are diminished; and that if, on the other hand, the upper end of the divided spinal cord is irritated below the point of section, the arterial pressure rises to its original level and the heart to its previous activity.

The leading experiment is as follows:—Two centigrammes of curare, dissolved in a cubic centimetre of water, are injected below the skin, and immediately after artificial respiration is begun. This dose is sufficient, as was first shown by V. Bezold himself, to paralyse the extremities so completely that neither stimulation of the cord nor of any muscular nerve produces the slightest contraction of voluntary muscles, while, as we shall see on another occasion, it is not sufficient to interfere with the action of the heart. Respiration of course ceases, but it is maintained, as I have said, mechanically, the means employed for the purpose being a pair of bellows the tube

of which communicates with a cannula adapted to the trachea of the animal.

The membrane between the atlas and the occipital bone having been previously exposed and one of the carotid arteries connected with the manometer of the kymograph, observations are taken of the arterial pressure and of the frequency of the pulse. This done the spinal cord is divided at the atlas. Immediately the rate of pulsation is diminished, say from 140 to 100, and after a few seconds the arterial pressure sinks, say from three or four inches, to one or two. Needles are then inserted into the spinal cord, one at the upper edge of the axis, both of which are connected with the secondary coil of Dubois' induction apparatus. At once the heart beats more frequently and vigorously and the mercurial column attains its former level.

The next step in the experiment is the destruction of the cerebro-spinal cardiac nerves. These nerves, as you know, reach the heart or leave it either through the vagi or the sympathetic.

The destruction of the nerves is best effected with the galvanic cautery, the action of which is more certain and more easily controlled than any other agent which could be employed. It answers the purpose so completely that in careful subsequent dissection it is found that every nerve is severed. As soon as the destruction of the nerves is effected the spinal cord is again excited, great care being taken that the strength of the current shall be the same as in the previous observation.

There are, however, in these and other respects considerable differences in the results observed in different animals, the conditions of which have not yet been determined.*

Upon another dog under partial anæsthesia, he divided with a fine curved scalpel the corpus striatum and optic thalamus on one side, the corpus callosum having previously been cut through. The electrodes were then placed on the convolutions above and behind the sylvian fissure. Contraction resulted, when the current was strong, not only in the fore leg of the opposite side, but also in the hind leg. In another experiment he removed the whole cerebral masses above the four varolii, and applied the electrodes to the surface of the section. Muscular contractions resulted, limited to the fore limbs, right and left.—*Lancet*, No. 2,630, p. 136.

Handbook Physiological Laboratory.

(165.)—[Some of the following experiments are quoted not to show extreme torture but regardlessness to suffering, as well as the minute character of directions given, to bring them within the capacity of young people.]

* This beautifully simple experiment we owe to Dr. Stricker, with whom I had the pleasure of repeating it here very frequently last summer.—(*Medical Times*, No. 1,094, pp. 683-4.)

Page 1.—Take the newt out of the water, dry the tail, cut off its end. If no blood comes, squeeze the organ from the root towards the tip until a drop is obtained.

Page 25.—To study the forms of the various cells of the separate layers we may obtain a thin shred from the surface of the tongue or gurus of a mammal by energetically scraping it with a scalpel.

Page 26.—Finally, if we have scraped very energetically with the scalpel, we meet with cells corresponding to the deepest layers. . . . A frog is held by an assistant, its nictitating drawn down, and from the anterior corneal surface a thin layer is scraped with a lancet-shaped or cataract knife.

Page 34.—It is not difficult to remove those structures [network of elastic fibres] even from the living animal. The easiest way is to place the vocal cord for a few minutes in dilute acetic acid, and then to scrape off the epithelium with a lancet-shaped needle—a process which is much facilitated by the previous steeping in the acid.

Page 35.—A frog is held by an assistant in such a way that the *bulbus oculi* is tense. The *membrana nictitans* is then drawn back, and the bulb penetrated with a cataract knife, just as in the operation for cataract, at the *limbus conjunctivæ* next the inner cauthus. The point of the knife is advanced until it approaches the *limbus* of the opposite side, without puncturing it, and is then carried outwards and upwards, so as to form a flap, consisting of the upper half of the cornea. The extreme edge of the flap must then be seized with the forceps, while the lower half of the cornea is cut away with the aid of scissors curved in the direction of their edge. The cornea is next transferred to a drop of humor aqueous (previously obtained by puncturing the opposite eye), and spread out on the glass slide with the anterior surface uppermost.

Page 38.—Preparations are obtained by stripping off shreds of a cornea (of a rabbit or frog).

The centre of the cornea of a frog, which is held by an assistant in the manner previously described, is firmly cauterised with a pointed stick of lunar caustic. One or two drops of salt solution are then allowed to flow over the cornea to decompose the excess of nitrate of silver. About an hour after the cauterisation the cornea is excised in the manner directed in p. 35.

It consists in first scraping the cornea of a living frog or small mammal with a sharp cataract knife so as to remove the epithelium completely. After a little practice, and provided the bulb is properly fixed by an assistant, it is not difficult to perform this operation without injuring the substance of the cornea. Thereupon the caustic is two or three times lightly rubbed over the whole surface, after which the eye is washed with saline solution, and the animal is left to itself for twenty or thirty minutes. The cornea is then excised.

Page 44.—If in a rabbit the skin and subcutaneous tissue are divided over the inner (anterior) third of the infraorbital edge, and

the thin membrane which stretches over the infraorbital fossa is severed. it is easy to remove, along with the glandula infraorbitalis, a gelatinous hyaline mass.

If the tail of a very young rat is amputated, and the tip torn asunder from the cut end, a great number of isolated lengths of tendon are obtained.

Page 54.—The leg of a water-beetle (*hydrophilus*) is torn out and its horny covering removed.

Page 60.—One of the hind-legs of a tadpole is amputated at the thigh. The animal is then replaced in water. After forty-eight hours the loosened muscular fibres hang from the stump in long pencils. These are cut off close to the surface of the stump with sharp scissors.

Page 78.—In a living or recently-killed rabbit the cornea is excised close to the limbus.

A silk thread having been passed through the centre of the cornea of rana esculenta and brought out again at the sclerotic ring, the two ends are knotted together. After the thread has remained from five to eight hours the cornea is excised.

Page 97.—In a large frog, secured on its back, the abdominal vein is carefully exposed, under a dissecting lens, in its course up the middle line of the anterior wall of the belly. A ligature is passed round the distal end of the prepared part and tightened.

Page 98.—The external jugular of the mammal is then exposed by a sufficient incision and cleared of the surrounding tissue with the aid of dissecting forceps.

Page 108.—A snip is made in the right side of the belly. The incision is then continued upwards and downwards in such directions as to avoid bleeding. The muscles are divided in the same vertical line. The intestine and mesentery are drawn out carefully.

Page 109.—The animal must be curarised as before the tongue drawn out by the cornua, around which a thread must be secured to pins.

Page 113.—In a large or middle-sized rabbit which has been kept from sixteen to twenty hours without food, ten cubic centimetres of a warm five per cent. solution of Prussian blue are injected into the abdominal cavity. After three hours and a half the animal is bled to death by opening the carotid artery, or killed by strangling.

Pages 158 to 162.—The inflammatory changes of the epithelial elements of the cornea may be studied by abrading the epithelium over a limited surface in several frogs, and examining the organ at various periods after the injury. After two or three days, sections may be made by shaving off a portion of the mucous membrane. In the blood vessels the inflammatory

changes may be studied by cauterising the external surface of any superficial vein (e.g., the external jugular or femoral), or even by simply ligaturing the vessel. Three or four days after the injury the vessel is excised. . . . The best method is to pass a needle into the knee-joint of a rabbit in such a way that it penetrates into the tibia. A few days after, sections are made of the fresh cartilage and stained in gold. . . . Germination of the cells of bone may be induced in the long bones of mammalia by passing a red hot needle as deeply as possible into a bone previously freed of the soft parts covering it, and then cauterising the hole with a pointed stick of nitrate of silver, or by violent fracture. After a week or more the bone is excised. . . . Inflammation of the tissue of the liver may be induced by passing a needle into the organ. Twenty-four to forty-eight hours after the injury the animal must be killed. . . . The cornea may be cauterised at the centre to such a depth as almost to perforate it, or a thread may be drawn through it, entering at the centre and passing out through the sclerotic beyond the margin, the ends of which are then tied. . . . To study the successive stages of the process, half-a-dozen corneas should be prepared in this way at a time, which can then be excised after eight, twelve, eighteen, twenty-four, thirty-six and forty-eight hours. The best preparations are obtained from *rana esculenta* during the summer months, from eight to twenty-four hours after the introduction of a silk thread as above described. . . . Inflammation is produced in one eye by cauterisation, and then twenty-four hours after a portion of the cornea of the other eye is excised, spread out carefully, and lodged between the membrana nictitans and the cornea of the injured eye. The membrana nictitans is then drawn up and secured by two or three ligatures to the skin. After twenty-four hours more the sac is opened and the cornea taken out. . . . The mucous membrane covering the large lymphatic sac of the under surface is snipped off with curved scissors. The observation is necessarily tedious, often lasting for forty-eight hours. It is therefore desirable to replace the tongue in the mouth for a time after each examination. . . . In a curarised tadpole the required degree of irritation can be produced either by simply pencilling the surface, or by allowing a drop of ammonia to fall on it from a capillary pipette, or, finally, by piercing it with a needle. The research must be continued often for many hours.

Page 166.—Several frogs are then selected, in each of which the pericardium is exposed, and divided as directed in § 46, and a snip made in the ventricle with fine scissors.

Page 174.—In a rabbit two small incisions are made across the course of the external jugular vein (*see* § 48), one near the clavicle, the other near the origin of the vessel, great care being taken not to go deeper than is necessary, in order to see the vessel through the

fascia. A small needle is then passed under the vein, near the proximal incision, in a direction at right angles to that of its axis, and corresponding to that of the incision, but deeper. A second needle is then laid in the course of the incision, and drawn tightly towards the first by a ligature at either end, by which means the blood-current is entirely arrested, while the coats of the vein are absolutely protected from injury. A second pair of needles is then inserted at the distal incision, and secured in a similar manner so as to shut in the blood with which the vein becomes distended after the tightening of the first ligature. After the lapse of a couple of days the ligatured portion of the vein is exposed at some part of its course, and punctured with a glass pipette, by means of which the blood is withdrawn from it by suction in a perfectly liquid state.

The arterial trunks leading from the heart of a frog or tortoise are first tied, and then (as soon as the heart has become distended) the venous trunks. The heart full of blood is removed from the body.

The pericardium of a frog is then exposed and divided, and a snip made in the ventricle with absolutely clean scissors.

Page 176.—A frog having been secured in the usual way (*see* § 46) in the prone position, the heart is exposed, and the right aorta ligatured. A clip is then placed on the left aorta at its origin from the bulb.

Page 212.—The animal having been secured on Czermak's rabbit board, and the fur clipped, the skin is pinched up between the finger and thumb on either side of the upper end of the trachea so as to form a horizontal fold, which an assistant divides vertically.

The opening having been enlarged with the aid of a second pair of blunt forceps, the sterno-mastoid is slightly drawn aside, so as to bring the artery with its three accompanying nerves, the vagus, the depressor, and the sympathetic, into view. The sheath having been opened the artery is raised on a blunt hook, and easily cleared from its attachments to a distance of three-quarters of an inch in either direction. When kept closed by the adjusting screw, seize upon the head of a cat or rabbit in such a manner as to hold it firmly without inflicting the slightest injury. The neck of the animal rests on a cylindrical cushion, covered with waterproof cloth, and the rest of the body on a mattress of similar material. Along the edges of the board there are convenient attachments for the extremities.

Page 229.—The excellency of this method lies in the fact that the animal can be kept under observations, without the use of any narcotising drug, for a long time in a perfectly natural condition. The frog is used both in the larval and adult state. To observe the circulation in the tail of the tadpole, the animal is placed in a moderately strong solution of curare, care being taken to remove it before it is completely paralysed—the moment, in short, that its

motions become sluggish. It is also possible to secure it, without the aid of curare, in a holder of construction similar to that of the instrument I have just described—a method which has this great advantage, that the animal is in a more normal condition; for even when curare is given with the greatest care, the action of the heart is weakened by it.

There are three transparent parts of the frog—the mesentery, the web, and the tongue—each of which has its special advantages for the purposes of study. For a first view of the relation between arteries, capillaries, veins, and lymphatics, the mesentery is superior to either of the other two. The frog must be placed under the influence of curare, the dose of which for the ordinary specimens of *rana temporaria*, is about one two thousandth of a grain.

The solution of curare is prepared by weighing out five milligrammes of the substance, and rubbing it up in a glass mortar with a little alcohol. The proper quantity of water—that is, sufficient to make up ten cubic centimetres, is then added, and a straw-coloured, nearly limpid liquid is obtained, a single drop of which is a sufficient dose. It is injected under the skin of the back with an ordinary subcutaneous syringe, and answers best when the effect does not manifest itself for some time after the injection.

Page 230.—The observation may be continued without material change for many hours.

Page 233.—All being now ready, a frog, previously slightly curarized, is fixed on the table in the supine position. The integument is divided over the sternum in the middle line, and the anterior wall of the upper part of the visceral cavity removed, so as to expose the pericardium, great care being taken not to injure the abdominal vein, or any other large vessel. The ventricle is then opened, and the cannula passed through the opening into the bulb and secured by a ligature. This done, the heart is drawn upwards.

Page 235.—Two frogs are slightly curarized, and placed side by side on the same board in the supine position. In both the heart and great vessels are exposed as in the preceding section. The brain and spinal cord are destroyed in one of the frogs by inserting a strong needle into the spinal canal immediately below the occipital bone, and then passing it upwards and downwards. [The other frog is left sensitive.]

Page 236.—Two frogs are suspended side by side, one of which has been pithed in the manner above described. In both the heart is exposed and the ventricle cut across.

Page 237.—A frog having been curarized just sufficiently to paralyse its voluntary muscles.

All being now ready, the integument is opened along the middle line of the back of the neck, and the occipital bone perforated in the middle line with a fine awl close to its posterior margin.

The frog is then laid, back downwards on the board, in such a position that one of the needles enters the cranium through the hole in the occipital bone, the other the spinal canal. . . . Finally, the heart is exposed as before.

Page 238.—The dose of curare must be very small, and should therefore be given an hour or two before the observation is made. One at least of the electrodes must be inserted within the cranium; for if both are below the occipital bone the effect is uncertain. . . . The integument is divided in the middle line from the pomum adami downwards, as directed in section 1. On drawing the edge of the incision to either side, the external jugular vein is readily seen as it crosses the sterno-mastoid. It is then carefully cleared of the platysma fibres and fascia which cover it, and of its sheath to the extent of an inch or more, with the aid of two pairs of blunt forceps. . . . The animal being under the influence of curare its voluntary muscles are paralysed.

Page 240.—The cannulæ having been placed in the trachea and external jugular vein, and the apparatus for artificial respiration being in order, three-tenths of a centimeter of a one per cent. solution of curare is injected. . . . With the help of this needle three ligatures are passed underneath the muscles which stretch vertically on either side of the spine of the atlas. . . . The ligatures having been tightened and the muscles divided in the middle line . . . all that is necessary is to divide carefully first the skin, and then the fascia which covers it. . . . To observe the effect of vascular contraction on the heart, that organ must be exposed. In a curarized animal this can be effected without interfering materially with the vital functions. Ligatures of fine copper wire having been passed with the aid of a curved needle around the 3rd, 4th, 5th, and 6th cartilages, close to the left edge of the sternum, and a second vertical series of ligatures around the corresponding ribs at a sufficient distance outwards, the portion of the thoracic wall which lies between the two series can be removed without hæmorrhage. It is then seen that after section of the cord the heart is flaccid and empty, and that its cavities fill, and its action becomes vigorous when the vascular contraction caused by excitation of the peripheral end forces the blood forwards, so as to fill the right auricle.

Page 242.—For this purpose the occipital bone must be perforated with a small trephine in the middle line between the occipital protuberance and the occipital spine. By this opening a thin-bladed knife is introduced in the middle plane with its edge outwards, by which the medulla is divided, first on one side, then on the other.

The currents employed must be feeble when the nerves are excited by the direct application of the electrodes to the sensory nerves, but strong when it is intended to excite their cutaneous or mucous

endings. The periods of excitation should always be very short. The experiment may be varied as follows:—A frog having been carefully curarized.

the points of the excitator are placed upon the tongue, the mouth being kept open for the purpose.

Page 243.—A frog having been curarized, the integument is divided along the outer and posterior aspect of the thigh in a line which corresponds in direction with the slender biceps muscle, or rather with the groove between the muscular mass which covers the front of the femur (triceps femoris) and the bulky semi-membranosus. The sciatic nerve, accompanied by the sciatic artery and vein, lies immediately underneath the biceps, between it and the semi-membranosus. In order to separate it from the vessels it is best to bring it into view by raising the biceps on a blunt hook.

The nerve is divided a little above the knee, and the central end laid on the copper points.

Page 245.—The animal having been curarized the apparatus for artificial respiration is connected with the trachea.

The great auricular nerve is then carefully exposed, separated from the surrounding parts with the aid of two pairs of blunt forceps, and divided.

If care is taken neither to prolong the excitation unduly, nor to use too strong currents, the re-action may be witnessed a great number of times in the same animal.

Page 265.—The heart of a frog having been exposed in the usual way, a stout glass rod is introduced into the œsophagus. All the other organs may now be removed in the manner directed in § 63, care being taken to avoid interfering with the venæ cavæ.

Page 269.—A frog having been slightly curarized

the sternum is then divided in the middle line, and the two halves of the wall of the chest drawn to either side, so as to expose the pericardium and lungs, while a stout glass rod is passed down the œsophagus.

Page 271.—The preliminary steps of the experiment are those described in § 34. Loose ligatures having been placed round both vagi, and a kymographic observation made to determine the normal arterial pressure and frequency of the pulse, both nerves are divided simultaneously [of a dog].

The contractions of the heart become so frequent that the oscillations can no longer be followed by the eye, all that can be distinguished being a vibratile movement of the column. On exciting the peri-

pheral end of either vagus, the same effects are produced as in the rabbit.

Page 272.—A frog is secured in the supine position. The pleuro-peritonæal cavity is then opened, and the intestines and other viscera are removed, great care being taken not to injure the mesentery, or the vessels and nerves which it contains.

Page 273.—To show this a frog is secured on its back, the pleuro-peritonæal cavity opened, and the heart exposed as before. The surface of the intestine is then smartly tapped.

If the ganglionic cord is then divided on each side opposite the junction of the two aortæ, and the experiment repeated, no effect is produced. Another frog is prepared in the same way, with the exception that both vagi are divided.

The same thing happens if, instead of dividing the vagi, the cord is divided immediately below the medulla.

In a rabbit the trachea is connected with the apparatus for artificial respiration, and the vagi are exposed in the neck. Thereupon the spinal cord is divided immediately below the medulla oblongata.

In the dog, section of the cord generally diminishes the frequency of the pulse. There is no such effect in the rabbit.

Page 278.—In a curarized rabbit, in which artificial respiration is maintained in the usual way, an incision is made in the middle line, extending from the upper third of the sternum to the upper end of the trachea. The external jugular vein of one side is then brought into view, tied in two places, and divided between the ligatures. The sterno-mastoid muscle is also divided between ligatures; a strong threaded aneurism needle is thrust under the sterno-clavicular ligament, and the upper fibres of the pectoral muscles; these with the ligament are divided between ligatures, and the cut ends drawn aside.

The superficial parts having been exposed by two lines of incision, one of which is in the middle line, while the other extend from it on either side in the direction of the sterno-clavicular ligament, and the jugular vein having been divided between ligatures, the next step is to find the pneumogastric nerve at the upper part of the wound, and free it from the surrounding tissues.

To find it, the most certain method is to seek for the trunk of the sympathetic in the upper part of the space, where it lies concealed behind the carotid artery, and then to trace it down to the ganglion. All this having been accomplished without bleeding, there is no difficulty in passing a ligature round the ganglion, so that at any desired moment it may be extirpated.

Both ganglia having been thus prepared with as little loss of time as

possible, the sympathetic and vagus are divided. The medulla oblongata is then divided.

Page 285.—Two frogs having been slightly curarized are prepared thus: the heart having been exposed *legè artis*, a small opening is made in the skin in the occipital region. In one of the frogs the brain and spinal cord are completely destroyed by passing a needle upwards and downwards from the occipital region, and then both are hung vertically on a board, side by side, looking in the same direction. [Sensibility is retained in the other.]

The discharge of sanguineous liquid goes on for one or two hours, and if during the progress of the experiment, the vasomotor centre is stimulated reflexly by exciting a sensory nerve on the surface of the skin, it is seen that the rate of flow is at once augmented, but becomes less after the cessation of the excitation than it was before.

Page 289.—This may be demonstrated graphically by puncturing the anterior wall of the visceral cavity, and introducing through the puncture a cannula in such a way that it communicates with the cavity of one lung.

Page 293.—A rabbit having been secured on the rabbit support, the skin is perforated with a scalpel close to the left edge of the middle of the sternum. This having been done, the point of the tube is easily passed into the right pleura by pushing it in a horizontal direction behind the sternum, with its point against the posterior, *i.e.* (as the animal is placed), the under-surface of the thoracic wall.

Page 297.—The student must avail himself of the excessive and infrequent respirations of animals in which both vagi have been divided.

Page 298.—The best view of the movements is obtained by dividing the hyothyroid membrane. The skin having been carefully divided in the middle line *legè artis*, the membrane must be exposed with the aid of two pairs of forceps.

Page 306.—A ligature is passed round each nerve a little below the cricoid cartilage.

Page 308.—Rabbits in which both vagi have been divided commonly die before the end of the first day. Dogs live longer, often two or three days.

Page 309.—By far the best method is to introduce into the peritoneal cavity, by means of a small opening in the linea alba, close to the ensiform cartilage, a small flat bag of india-rubber of such size that it can be conveniently slipped between the diaphragm and liver.

Page 312.—*Excitation of the superior laryngeal nerve.*—The experimental investigation of the superior laryngeal is much more difficult than that of the trunk of the vagus, partly because the nerve is difficult to reach and runs a short course, partly because it is very slender. To expose it in the rabbit, an incision should be made extending from the side of the trachea at the level of its first and second rings to the hollow between the angle of the jaw and the

larynx. After severing the skin in the usual way, the fascia which extends forwards from the edge of the sterno-mastoid muscle must be carefully broken through with the aid of two pairs of dissecting forceps, so as to expose the parts seen in fig. 227. The space is divided into two by the artery, the direction of which coincides exactly with that of the original incision. Near its lower end the artery gives off its thyroid branch. At the top the space is limited by the tendon of the stylohyoid muscle, and the posterior cornu of the hyoid bone. Immediately below the muscle is the trunk of the ninth nerve which arches forwards towards the tongue. The descending branch of that nerve passes downwards and forwards to reach the muscles which cover the front of the trachea, giving communicating branches to the cervical plexus, and a branch which arches forwards over the artery to gain the muscles which draw the larynx upwards.—Before proceeding to expose the deeper nerves, it is well, in order to avoid confusion, to remove the *descendens noni*; the next step is to draw the larynx well to the side opposite to that chosen for the incision, so as to widen the space between it and the carotid artery. This done, the exposure of the superior laryngeal becomes easy. Its exact position is indicated in the figure; its course is much twisted so as to allow of the up-and-down movements of the larynx. In preparing it, no cutting instruments must be used. It must be freed from the surrounding structures with the aid of two pairs of forceps, any veins in the way having been divided between two ligatures. Care must be taken, however, to leave a certain quantity of cellular tissue about it to serve as a kind of protective sheath, and make it somewhat less liable to get dry. The nerve having been prepared, a ligature must be tied round it as near as possible to the thyrohyoid membrane, after which it must be divided beyond. In the dog or cat the mode of preparation is very much the same as in the rabbit. In the cat, the comparative thickness of the nerve facilitates the manipulation.

In exciting the superior laryngeal, the great source of difficulty is the proximity of the vagus and the consequent liability of that nerve to be acted on by the induced current in a unipolar way. This accident, which is of course fatal to the success of the investigation, the functions of the two nerves being opposite, is to be avoided, not by the use of complicated arrangements for the insulation of the nerve, but by placing it in such a way on the ordinary copper points that the part acted on is separated by a considerable air space from the surrounding tissues. Before beginning the excitation, the secondary coil must be shifted to a distance from the primary, and the primary current divided by means of Helmholtz's side wire into two branches, one of which only passes through the breaker. The other is led directly from the battery to the coil, so that the primary current is never entirely opened. In this way the opening induction shock, which, in the ordinary arrangement of the induction apparatus,

possesses a much greater tension than that of the closing shock, is so reduced that the two become nearly equal to each other.* Consequently, as the risk of unipolar action varies with the maximum intensity of the current, it is very much diminished by this contrivance—so much so, indeed, that if care is taken to prepare the nerve properly, even moderately strong currents may be used without any efforts referable to unipolar excitation of the vagus manifesting themselves.—Excitation of the central end of the superior laryngeal produces, according to the strength of the current used, either diminution of frequency of the respiratory movements or complete relaxation of the muscles of inspiration. The most advantageous way of judging of its effect on the diaphragm, is to expose that muscle in the way directed in § 91. It is there seen that that muscle becomes absolutely flaccid during excitation of the nerve, and it is drawn up by the elastic contraction of the lungs, so as to assume its highest possible position. When the excitation is discontinued, the relaxation either gives way to natural breathing or is immediately succeeded by one or two vigorous inspirations. If the current is so feeble that it merely diminishes the frequency of the respirations, without arresting them, the tracings show that there is no diminution of the duration of the inspiratory acts, and that the slowing is entirely due to a prolongation of the intervals, *i.e.*, of the periods during which the diaphragm remains in the position assumed by it at the close of ordinary expiration. To record the effects graphically, any of the methods recommended in the preceding paragraphs may be used. If the method described in § 99 is employed, a tracing is obtained which exactly resembles fig. 255.

The tracing fig. 256 † was drawn by inserting a bag between the diaphragm and the liver.

Page 316.—The experiment by which it is proved that the respiratory phases of arterial pressure and pulse frequency are independent of the thoracic movement consists in curarizing a dog by the injection into the venous system of a dose of solution of curare only just sufficient to paralyse the respiratory muscles.

Page 319.—For this purpose a cannula must be fixed air-tight in the trachæ.

Excessive respiratory movements in which at first the expansive efforts of thoracic muscles, afterwards the expulsive efforts of the muscles of the abdominal wall are most violent.

* For a fuller explanation of the difference between the two induced currents and of the effect of Helmholtz's modification, see Rosenthal, "Electricitätslehre," p. 120.

† The tracing fig. 256, shows that during the whole period of excitation the diaphragm remained motionless in the position of expiration, with the exception that at gradually lengthening intervals it executed momentary contractions. When after the cessation of excitation the respiratory movements were resumed, they were slower but more extensive than before.

Towards the close of the first minute the animal becomes convulsed. These convulsions must be attentively studied, because they are the type by comparison with which all other convulsions of the same order are described or defined.

Afterwards the contractions of the proper expiratory muscles are accompanied by more or less irregular spasms of the muscles of the limbs, particularly of the flexors. Early in the second minute the convulsions cease, often suddenly; simultaneously with their cessation, the expiratory efforts become indistinguishable, and the animal lies in a state of tranquility, which contrasts in the most striking way with the storm which preceded it. In these spasms which accompany the final gasps of an asphyxiated animal, the head is thrown back, the trunk straightened or arched backwards, and the limbs are extended, while the mouth gapes and the nostrils dilate.

Page 321.—The respiratory movements, at first natural, are gradually exaggerated, both as regards their extent and frequency. Towards the end of the period, as in the former case, the expiratory movements gain in vigour, both absolutely and relatively to those of inspiration, so that each inspiratory act is immediately followed by a sudden tightening of the anterior abdominal wall, accompanied by convulsive twitchings of the limbs. Suddenly the violent expulsive efforts cease and the inspiratory movements assume the character already described, consisting in spasmodic contractions of the diaphragm, accompanied by gasping movements of the head and neck.

Page 322.—During the convulsive struggle, and particularly towards its close, the heart enlarges to something like the double of its former dimensions,—this enlargement being due to the lengthening of the diastolic interval, and to the quantity of blood contained in the great veins, which in fact are so distended that if cut into they spirt like arteries. The effect of these changes on the arterial pressure can be best studied in a curarized animal.

Page 363.—Poison a frog completely with urari Dissect out carefully one of the large muscles of the thigh Cut away with it the piece of the pelvis to which its origin is attached.

Page 395.—Introduce between the skin of the back of a strong frog a drop or two of a solution of urari.

. In a short time the frog will be found perfectly motionless, with its respiration arrested, but its heart still beating.

Lay bare the sciatic nerve in the thigh, slip under it a pair of electrodes connected with an induction coil and stimulate the nerve with an interrupted current. If the animal has been thoroughly poisoned, no contractions what-

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Lay bare the sciatic nerve in the thigh, slip under it a pair of electrodes connected with an induction coil and stimulate the nerve with an interrupted current. If the animal has been thoroughly poisoned, no contractions what-

ever in the muscles of the leg will follow upon the application of a stimulus.

If contractions do make their appearance, the poisoning is not complete; and the student must wait or inject a further quantity of the poison.

Lay bare any of the muscles of the leg and apply the electrodes directly to them. Contractions will be manifest upon the application of a very slight stimulus.

In a strong frog make an incision through the skin between the ilium and coccyx along the line *k.m.* fig. 266. Cut cautiously through the ileo-coccygeal muscle until the peritoneal cavity is reached. The three nerves which go to form the sciatic nerve will come into view when the sides of the wound are held apart. Very cautiously, by means of a small aneurism needle, pass a thread under these nerves, putting it under from the outside and bringing it out again on the median side.

Repeat the same process on the other side, passing the same thread under the nerves of that side too, but putting it in at the median side and bringing it out at the side.

Tie the thread very tightly round the abdomen so as to check entirely the flow of blood to the lower limbs. All this may be done under a slight dose of chloroform.

Pinching or otherwise stimulating either hind foot may produce movements in either one or both hind limbs, but in no other parts of the body.

Pinching or otherwise stimulating the skin of the head, fore limbs or trunk above the ligature may produce movements in the hind limbs, but in no other part of the body.

These facts are intelligible only on the hypothesis that the urari has destroyed (or suspended) the irritability of the motor nerves in that part of the body to which, by means of the blood current, it has had access, but has not destroyed the irritability of the sensory nerves, or of the central nervous system. Pinching the skin of the fore limb gave rise to an afferent nervous impulse, which, either by volition, or by reflex action, gave rise in turn to efferent impulses, which were unable to manifest themselves through the poisoned motor nerves of the fore limbs and trunk, but found vent through the unpoisoned motor nerves of the hind limbs. In order to bring out these results well, the dose of poison must not be more than sufficient to poison the motor nerves.

In a fresh strong frog lay bare the sciatic nerve on one side,

place a ligature under it, near where it divides into two branches, and tie the ligature tightly round the leg above the knee. The circulation of the lower right leg will thus be completely arrested; but, inasmuch as the nerve is not included in the ligature, there will be complete nervous connection between the right lower leg and the rest of the body. Poison with urari.

In a fresh strong frog dissect out a gastrocnemius (or any other single muscle), dividing both insertion and origin, and ligaturing its blood vessels, thus leaving it connected with the rest of the body by its nerve only. Poison the frog with urari.

Page 400.—The results are most clear and distinct when the organs of consciousness are intact, and the ordinary tokens of sensation are used to determine whether the impulses caused by stimulation of the peripheral terminations reach the conscious central nervous system or not. But the facts may also be readily shown in the absence of the brain, when reflex action is taken as a proof of a centripetal impulse having reached the spinal cord. In the former case, the frog should be placed under chloroform during the laying bare of the roots. In the latter the medulla should be previously divided in the neck. The frog being placed on its belly, make an incision in the middle line of the back, from the upper end of coccyx to the level of the fore limbs. Having hooked back the flaps of skin, carry the median incision down to the spines of the vertebræ, and dissect away the longitudinal muscles on either side, so as to lay bare the bony arches, and then hook back the muscles on either side, or cut them away altogether.

With a small but strong blunt-pointed pair of scissors cut through, on either side, the arch of the last (eighth) vertebra (be careful not to thrust the scissors in too deep), and remove the piece so loosened. Proceed then to the next arch above and so remove, three arches. The roots of the nerves will be seen lying in the spinal canal. Snip away the remains of the arches, on each side, until the last three (or four) roots are quite clear, being very careful not to touch the nerves with the scissors. The frog being completely at rest, draw the ligature tight, observing the frog all the while. If the animal be in good condition, some movements will be visible in some parts of the body as evidence either of sensibility or reflex action. Now cut the nerve between the ligature and the cord: some movement will probably be again witnessed.

Lift the peripheral stump of the nerve carefully up by means of the ligature, and slip it upon the curved shielded electrodes which may be held in the hand, or, better, fixed on a moveable stand. To prevent any escape of the current, slip a fragment of india-rubber sheeting beneath the nerve and electrodes, so as to isolate these from the cord and from the rest of the nerves. Pass a moderately strong interrupted current through the electrodes. If there be no escape of the current the animal will not move in the slightest.

Repeat the observation with the nerve root next above (the 8th), with this difference: place the ligature as near as possible to the walls of the spinal canal; divide the nerve between the ligature and the wall, and place the central instead of the peripheral stump on the electrodes.

Ligature and section as before produce movements. A very

moderate current applied to the central stump will produce very considerable movements in various parts of the body, *i.e.* signs of sensation or reflex action, as the case may be.

Ligature or section of the posterior roots of spinal nerves produces movements in various parts of the body. Stimulation of the peripheral stump produces no movement whatever; stimulation of the central stump produces considerable movements.

These movements, be they simple reflex actions or more complicated voluntary movements set going by conscious sensations, are evidences of centripetal sensor impulses, excited in the posterior sensory roots.

Page 402.—In a fresh strong frog lay bare the roots of the spinal nerves and divide the posterior roots of the seventh, eighth, ninth and tenth nerves on the right side, and the corresponding anterior roots on the left side.

The left leg will remain motionless, being simply dragged along by the rest of the body, but never moving of itself. (If the brain has been previously destroyed or separated from the spinal cord, the right leg will be drawn up as usual, but not the left leg.)

Page 403.—Recurrent sensibility. [This is never witnessed in the frog. It can only be shown in the higher animals, the cat or dog being best adapted for the purpose. The method adopted is very similar to the above: the arches of one or two vertebræ being carefully sawn through, or cut through with the bone forceps, and the exposed roots being very carefully freed from the connective tissue surrounding them. If the animal be strong, and have thoroughly recovered from the chloroform and from the operation, irritation of the peripheral stump of the anterior root causes not only contractions in the muscles supplied by the nerve, but also movements in other parts of the body indicative of pain or sensations. On dividing the mixed trunk at some little distance from the junction of the roots, the contractions of the muscles supplied by the nerve cease, but the general signs of pain or of sensation still remain.

Page 411.—In a frog with divided medulla, ligature the hind limbs, leaving the nerves free, as directed in chap. 31, for urari, and afterwards inject a small dose of strychnia.

Demonstration of the functions of the chorda tympani and sympathetic fibres of the submaxillary gland in the dog. The animal was placed under the influence of the chloroform, when no less than fifteen different dissections were made to obtain a fistulous opening. After which the author says, p. 473, "The animal must previously be allowed to recover from chloroform, or no increase [of saliva] will be observed."

Page 474.—After division of both nerves the secretion of the submaxillary gland, which in the normal state only goes on when the gland is directly or reflexly excited, becomes constant and abundant. This effect does not occur until some time after section, and may

last for days or weeks. A similar condition of the gland is produced by the introduction of curare into the blood which is supplied to the gland by its arteries.

Page 477.—Give the dog a hearty meal, so as to distend its stomach completely, and make it lie close against the intestinal walls. Anæsthetize the animal by chloroform, taking care that the vapour is mixed with a sufficient proportion of air. Lay it on its back on the table; shave off the hair from the epigastric and hypochondriac regions, and remove the hairs carefully by a sponge, so as to prevent the risk of their getting into the peritoneal cavity. Make a vertical incision, about an inch and a half to one side of the linea alba, preferably the left, and parallel to it, extending downwards from the lower edge of the costal cartilages to a distance somewhat less than the diameter of the flange of the cannula. Divide the muscles parallel to the course of their fibres. Tie every bleeding point before opening the peritoneum, so that no blood shall get into its cavity. Open the peritoneum on a director. Lay hold of the stomach with a pair of artery forceps, at a point where there are not many vessels, and draw it forwards. Pass two threads with a curved needle into the gastric walls, at a distance from each other about equal to the diameter of the tube of the cannula, and bring them out again at a similar distance from the points where they were introduced. Make an incision into the gastric walls, between the two threads, rather shorter than the diameter of the tube of the cannula. Put a pair of forceps, with the blades together, into the incision, and then dilate it by separating the blades till it is large enough to allow the cannula to be introduced. Push the cannula into the stomach up to its outer plate. Tie the stomach to it by the threads, and then pass their ends through the edges of the wound in the abdominal wall in such a way as to fasten the stomach to it, and at the same time to keep the cut edges in apposition. No other suture is required. Leave the cannula uncorked for at least half an hour after the operation is finished, for when the dog recovers from the chloroform it will vomit, and if the cannula be corked the fluid contents of the stomach are apt to be forced past the side of the cannula into the abdominal cavity. Feed the dog on milk for one or two days, and if the operation be performed in winter, keep it in a place warmed night and day. The day after the operation the edges of the wound will be much swollen, but the swelling will subside in a day or two. After the wound has begun to heal, the cicatrix may thicken, and the outer plate of the cannula begin to press too much on the skin, so that it ulcerates. If this should occur, the cannula must be lengthened by screwing the two flanges further apart. The cannula may be closed by an india-rubber stopper, or by a cork. If the dog tears out the cork with his teeth, soak it in decoction of colocynth, or put a little phosphoric acid on its outer end. In order to collect the juice, let the animal fast for several hours, so that its stomach

may be quite empty, but not for more than a day, as the mucus membrane would become covered with a thick coating of mucus. Let an assistant pat the dog and keep him quiet, withdraw the cork from the cannula, and tickle the inside of the stomach with a feather tied to a glass rod. Put a small beaker underneath, so that the end of the rod rests on its bottom: the gastric juice will flow into it down the sides of the rod.

Page 494.—This may be still better seen in the cat. To show these facts a cat must be placed under chloroform, after which both vagi are prepared and the stomach exposed. If now the animal having partially recovered from the anæsthetic, the stomach is seized between the thumb and forefinger, and subjected to traction in the direction of its length, slight but unequivocal signs of uneasiness are perceived. The vagi are then divided.

Page 499.—To get the urine, hold the rabbit over a large beaker, compress the abdomen with the palm of one hand, and press with the thumb of the other on the bladder just above the pubes, pushing it well down into the pelvis.

Page 505.—Chloroform the animal, and secure it on the rabbit-support. Make an incision from an inch to an inch and a quarter long through the abdominal parietes in the linea alba from the xiphoid process downwards. The pyloric end of the stomach is thus exposed. Pull gently on the stomach until the duodenum is brought into view. The part corresponding to the superior transverse part in man forms a loop with its convexity directed towards the diaphragm, into the top of which convexity the ductus choledochus enters. Tie the duct in this situation, then seize the gall bladder with a pair of forceps. It is always full, and cannot be missed if the forceps are passed immediately under the edge of the costal cartilages. Make a small incision into the gall bladder, introduce a cannula and tie it in. The diameter of the cannula should be from two to three centimeters, and the end to be inserted should have a projecting rim. This can be made very readily by heating the end of a piece of glass tubing of the proper size, and pressing it, while hot, against a flat piece of iron. Sew up the wound, leaving the free end of the cannula outside. The bile in guinea-pigs is secreted in very large quantities, being as much as 7·3 grammes in an hour per kilogramme of body-weight. It contains a very small proportion of solids, about 1·3 per cent. When the bile duct is tied the guinea-pigs die in less than twenty-four hours, but when it is not tied they will live for a week.

Page 515.—Place a rabbit in the prone position on Czermak's rabbit-support, and fix the head to the upright at the side. Feel for the occipital protuberance, and make an incision over it about half an inch long. Fix the point of the chisel in the middle line of the skull, just behind the protuberance, and bore through the bone, moving the handle of the instrument from side to side, in order to assist its passage, but not pressing with too great a force. When the

skull has been penetrated, push the chisel downwards and forwards through the cerebellum in such a direction as to cross a line joining the two auditory meatus until it is stopped by the basilar process, and then gently withdraw it. Remove some of the urine in half an hour or an hour afterwards, and test it for sugar.

Page 516.—Bernard prefers for the purpose large dogs, sheep dogs being the best, as they are less subject to peritonitis than others. Five or six hours before the operation the animal should get a large meal of bread and meat. The operation, which must be performed as quickly as possible, consists in laying the dog on its left side, and making an incision five centimeters long in the right hypochondrium.

The duodenum lies opposite the wound. As soon as it is exposed it is drawn out.

The duct is opened with scissors, and a plain silver cannula, about five millimetres in diameter and ten or twelve centimetres long, pushed into it up to its first division.

Page 517.—For permanent fistulæ Ludwig and Bernstein choose small dogs, as in them the duodenum is more easily reached from the middle line, and is not drawn so far from its natural position by the fistula as in larger animals. The dog must be kept fasting on the day of the operation, as the pancreatic vessels are full during digestion, and bleed easily. Narcotize the animal by injecting opium into the tibial vein, and open the abdomen by an incision about two centimeters long in the linea alba, midway between the ensiform cartilage and the umbilicus. The duodenum is then searched for and drawn out of the wound along with the attached pancreas, and a thread looped round the duct. Instead of then putting in a cannula a piece of lead wire is inserted into the duct, so that one end of it passes into the intestine, and the other into the gland to a considerable distance. The middle part of it is twisted together and projects through the wound. Owing to the T shape thus given to the wire, it cannot either slip out or move about in the duct; but wire being chosen which does not fill it up, the flow of the juice is not hindered. Three threads having then been passed through the wall of the duodenum near the duct, the intestine and omentum are replaced in the abdomen, and the duodenum fastened by the threads to the abdominal wall. The wound is then sewed up, care being taken that the twisted part of the lead wire passes through the wound. Twenty-four hours after the operation the stitches are taken out, but the wire left in. In two or three days afterwards the juice can be collected. For this purpose the animal must be supported by two straps which pass under its belly, and are attached to a horizontal bar hung from the roof by a cord and pulley. The dog is thus suspended over a table, at such a height that it can barely touch it with its toes, in which position it remains perfectly still.

[The second volume of this work consists of elaborately-drawn plates and keys designed to assist "beginners" in manipulation.]

(165*a*).—[The following advertisement has been published by Longman & Co.:—]

Mr. Cooke's Classes of Anatomy and Physiology serve to complete and illustrate the Tablets. They are rapidly conducted, yet the instruction given is complete and exhaustive. The whole of Anatomy (on the dissected body) and of Physiology (with microscopical preparations and vivisections) are gone through carefully every three months; and the principal points are again glanced at during the fortnight which precedes each examination.

The object of the classes is to enable students to add, to sound practical knowledge previously and slowly acquired, that readiness and precision in details, which are daily becoming more requisite at examinations. The classes are not intended to help idle students to make up, by any artificial method, for time previously mis-spent.—[Address:] T. Cooke, Esq., M.D., 30, Gower Street, Bedford Square.

(165*b*).—At a recent visit to the Brown Institution I saw several animals kept for experiments, and also apparatus for the use of the operators. I was told by Dr. Burdon-Sanderson that one of the teachers performed experiments then before students to teach them physiology.—*Fohn Colam*. [See also p. 43.]

(*d*.)

OPINIONS MORE OR LESS AGAINST VIVISECTIONS.

(166).—Sir Charles Bell writes to his brother in 1822, "I should be writing a third paper on the nerves, but I cannot proceed without making some experiments which are so unpleasant to make that I defer them. You may think me silly, but I cannot perfectly convince myself that I am authorised in nature or religion to do these cruelties; . . . and yet what are my experiments in comparison with those that are daily done, and are done daily for nothing?"—*Spectator*, Feb. 6, 1875, p. 167, No. 2,432.

(167).—Sir Charles Bell has left it on record in reply to foreign writers who had improperly quoted him as an advocate of vivisection, "In a foreign review of my former papers the results have been considered in favour of experiments (on living animals). They are, on the contrary, deductions from anatomy, and I have had recourse to experiments, not to form my opinions, but to impress them on others. It must be my apology that my utmost powers of persuasion were

lost while I urged my statements on the ground of observation alone."

"Anatomy is already looked on with prejudice; let not its professors unnecessarily incur the censures of the humane. Experiments (vivisections) have never been the means of discovery, and the survey of what has been attempted of late years will prove that the opening of living animals has done more to perpetuate error than to enforce the just views taken from anatomy and the natural motions." —*Times*, August 13, 1863.

(168.)—What, then, is the real question at issue? It is whether or not it is cruelty to practise vivisection for the sake of advancing physiology, or, in other words, for increasing the means of detecting and curing disease. To such a question I answer that it is not cruelty at all, but a use of the lower animals, which is not only justifiable but imperative as a duty on all those who are skilled in this branch of human knowledge. Nevertheless, I hold that this can be done by the sacrifice of fewer animals than is the custom in continental schools of medicine. *I like to think that Sir Charles Bell ONLY EXPERIMENTED ON ONE LIVING RABBIT TO ESTABLISH HIS GREAT DISCOVERY.* That sacrifice was necessary, and he made it, but he kept just within the line of necessity, and thereby in recent times his fame as a discoverer has been endangered by claims put forward in favour of Magendi, who experimented more freely, and had the appearance of originating while he only corroborated the law of Bell.

1. Vivisection should only be performed for good and definite purposes, and as sparing as possible.

2. In all cases where painful operation is necessary, it ought to be performed while the animal is insensible.

3. It ought always to be performed by the Professor or other skilled person, and never by students.

4. Vivisection, while most necessary for the discovery of new facts, may sometimes be employed in order to demonstrate old but important truths to students when it is considered essential for their better understanding of disease and its treatment. I am, &c.,—EBEN. WATSON.—Letter of *Dr. Watson* to the *Glasgow Herald*, April 13th, 1875.

(169.)—I have long been of Sir Charles Bell's opinion, that vivisection is a delusion as a means of scientific progress. Of course its results, like any other set of facts, constitute a science in themselves, so do the results of murder, and so do the results of picking pockets—an exact science, if you like; and the earlier parts of the science will of course be subject to correction by the later: and thus vivisection may show, and has shown, truths and errors in the special walk of

vivisection. The science of animal agonies, like all sciences, can be corrected, eliminated, and completed by experiments of fresh and ever fresh agonies. But it has been a mistake to suppose that we were in the path of the humane sciences—in natural physiology, natural symptomatology, or within millions of leagues of medicine, when with rack and thumbscrew, and all torture, we were the inquisitors of the secrets of animal life.

Vivisectional anatomy has contributed to medicine, meaning by medicine, healing diseases, virtually nothing but “false paths and wrong roads.”—*Dr. F. F. Garth Wilkinson*, letter to the *Morning Star*, August 20th, 1863.

(170.)—There is one point which I feel it a duty to advert to. Mr. Hunter, whom I should not have believed to be very scrupulous about inflicting sufferings upon animals, nevertheless censures Spalanzani for the unmeaning repetition of similar experiments. Having resolved publicly to express my own opinions with respect to this subject, I choose the present opportunity to do it, because I believe Spalanzani to have been one of those who have tortured and destroyed animals in vain. I do not perceive that in the two principal subjects which he sought to elucidate he has added any important fact to our stock of knowledge; besides, some of his experiments are of a nature that a good man would have blushed to think of, and a wise man would have been ashamed to publish; for they prove no fact requiring to be proved, and only show that the aforesaid Abbé was a filthy-minded fellow.

The design of experiments is to interrogate nature; and surely the inquirer ought to make himself acquainted with the language of nature, and take care to propose pertinent questions; he ought further to consider the probable kind of replies that may be made to his inquiries, and the inferences that he may be warranted in drawing from different responses; so as to be able to determine whether by the commission of cruelty he is likely to obtain adequate instruction. Indeed, before we make experiments on sensitive beings we ought further to consider whether the information we seek may not be attainable by other means. I am aware of the advantages which have been derived from such experiments when made by persons of talent, and who have properly prepared themselves, but I also know that these experiments tend to harden the feelings, which often leads to the unnecessary and inconsiderate performance of them. Surely we should endeavour to foster, and not to stifle benevolence, the best sentiment of our nature, that which is productive of the greatest gratification and advantage both to its possessor and to others. Considering the professors in this place as the organs of the court of the college; addressing its members I feel that I act as becomes a senior of this institution, whilst admitting the propriety of the practice under the foregoing restrictions, I at the same time express an

earnest hope that the character of an English surgeon may never be tarnished by the commission of inconsiderate or unnecessary cruelty. I need not, gentlemen, caution you, who must feel anxious to maintain the respectability as well as the reputation of the medical character, against publishing experiments disgusting to common decency.—*Dr. Abernethy's Lecture*, No. 4, pp. 164-166.

(171.)—Mr. Hunter's opinion of life having the power of regulating temperature was deduced, not only from his experiments related in the *Philosophical Transactions*, but also from observing that in certain affections of the stomach the heat of the body is subject to great vicissitudes, whilst respiration and circulation remain unaltered; and also that parts of the body are subject to similar variations, which appear inexplicable upon any other supposition than that of local nervous excitement or torpor, or some similar affections of the vital powers of the part which undergoes such transitions.—*Abernethy's Lecture*, No. 1, p. 31.

(172.)—Much valuable labour has recently been expended in the investigation of the physiological action of drugs by means of experiments on the lower animals, and much that is helpful and suggestive in our clinical work has been derived therefrom; but there has been growing up a tendency, which seems to me to need reproof, to regard such a mode of investigation as the only one which should be called "scientific"; so that, while no "scientific" reputation is awarded to the most patient and astute interrogations and observation of clinical facts—a process which may call forth all the highest qualities of the mind—a limited number of experiments on the lower animals, the true value of which I shall immediately attempt to estimate, is sufficient to procure a reputation as an original investigator. Let us, by all means, and with all heartiness, give honour to real work of all kinds; but do not let us fall into the serious mistake of estimating the scientific value of an investigation by its remoteness from practical utility. The observation of the action of medicines on the lower animals is a considerable aid to clinical work, but it serves rather as affording hints as to the lines of investigation which should be followed by the clinical physician, than as giving him definite knowledge upon which he can safely act. Unfortunately, most of the animals upon which we are able to perform an experiment are very differently affected by certain drugs from what human beings are. Belladonna, stramonium, and hyoscyamus, for example, potent drugs when admitted into the human body, may be eaten by rabbits with impunity. Goats seem insensible to the action of tobacco, which is, as you know, a fatal poison to man when it is taken into his system, even in small quantity. Dogs are singularly insensible to the action of aloes; two or three grains will produce a purgative effect on most of our patients: it takes fifty or

sixty grains to act in the same way on a dog. On the other hand, half the amount of our ordinary doses of calomel will produce serious disturbance in the constitution of that animal. Again, opium and most other narcotics exert less soporific influence on the lower animals than on man. In the experiments I have recently been making with croton-chloral, the difference in its effects on the dog and the cat were quite remarkable: from fifteen to twenty grains proved a fatal dose to cats, while sixty grains merely sent a dog to sleep for less than two hours. Tartar emetic has scarcely any physiological effect on horses and cattle, but a few grains cause immediate vomiting in a dog. These and other facts of a like kind teach us to be exceedingly cautious in our inference as to the action of medicines on the human subject from the result of experiments on the lower animals.—*Dr. F. B. Yeo, British Medical Journal*, No. 699, p. 671. (*Clinical Lecture*). [See also *Langley*, No. 45; *Harley*, No. 97; *Marcd.*, *Thorowgood*, and *Lancet*, No. 126; *Reynolds*, No. 175; and *Moore*, No. 183.

(173.)—[After reciting a large number of experiments by different persons and at different places by giving mercury to dogs, Dr. Murchison says:—] On the other hand, it has been fairly objected that the results of experiments with mercury upon dogs do not warrant conclusions as to its effects upon man.—*British Medical Journal*, No. 696, p. 569.

(174.)—Mr. Wagstaffe thinks that experiments upon dead subjects, or even living animals, cannot be of much value in determining the question, for the conditions are essentially different from those which exist in man at the time when a rupture occurs.—*British Medical Journal*, No. 692, p. 454.

(175.)—Again, the inferences that have been drawn from observations on animals, with regard to the phenomena of diseases apparently common to those animals and to man, have, I think, been pushed too far, and have led to much unintelligible controversy. The differences between the human and the animal organisation have been sometimes lost sight of, although many of them are obvious enough.

Again, the action of many familiar drugs upon the most familiar animals is so different from that which we know to occur in man, that therapeutic experiments upon the former should be received with much more than "one grain of salt" when they are put forward as our guides in the treatment of human malady.—*Mr. Reynolds, British Medical Journal*, No. 711, p. 203.

(176.)—Sir,—To inflict pain for the purpose of establishing an

important truth, or testing an hypothesis, is one thing; to repeat such experiments over and over again, for the purpose of astonishing a class of students is another, the truth or the hypothesis having been confined or destroyed, and can only tend to tantalise the performer and the spectators.

This opinion is held by members of our profession whose names would carry enormous weight with them; and I conceive that neither science nor humanity would suffer if the law stepped in and sternly forbade the endless repetition of ruthless acts of cruelty for the mere purpose of exhibiting truths already known and admitted.—*Frederic D. Dyster, M.D., British Medical Journal*, No. 734, p. 126.

(177.)—I reminded Dr. Sanderson when he brought forward his paper on pyæmia; he appeared to be ignorant of those experiments. He inoculated rabbits with the blood of sheep that had died of this disease, and he went on killing rabbits within a given time, almost to the hour; he inoculated fourteen or fifteen in succession, and they all died within a certain time. In all those cases bacteria were found, and without bacteria being present he could not produce these fatal results.—*Dr. Crisp, British Medical Journal*, No. 747, pp. 558-9.

(178.)—In other cases the body of a living animal is made the subject of experiment, which sometimes implying neither pain or danger, amounts in other physiological inquiries to the infliction of agony and death in a manner which naturally makes one reluctant to employ it. Such experiments, indeed, it will be our object to collate and consider, rather than to repeat or imitate; the more so that the results they afford are often rendered equivocal or invalid by the circumstances which attend their infliction; by the agony, for instance, which deadens the sensibility of the unhappy creature to a degree such as suppresses all ordinary manifestations of feeling, or, conversely, provokes a series of movements or of inflammatory and other changes which mask the phenomena that would otherwise naturally ensue.—*Dr. Brunton, Lancet*, No. 1,941, p. 456.

(179.)—Pure physiology again, unassisted by comparative anatomy, has made much of pure function; but much as has been attempted in the way of experiment with infusions of pancreatic substance, and with the introduction of cannulæ into the duct of the gland, I am inclined to think that a comparison of the relative size of the gland in the carnivora and the herbivora respectively—in a dog, say, and in a rabbit—points as unmistakably as any of the lines of experiment just referred to, which, by the very nature of the case, are greatly beset with several sources of fallacy, to the fact that this salivary gland is concerned as much with the digestion of albumen and fat

as with that of starchy substances.—*George Rolleston, M.D., F.R.S., Linacre Professor of Anatomy and Physiology. Oxford; Lancet, No. 2,345, p. 182.*

(180.)—Every educated medical man knows that physiology is “the knowledge of normal functions of a normal body,” in other words, the knowledge “of the uses of the organs of a healthy body,” and it is on the very ground that every injury done in experiments on the living to a certain extent causes a deviation from the healthy condition, that deductions from such experiments have always in them something creative of doubt.

Everyone who has studied the subject of experiments on the living knows that numerous experiments have been tried on dogs in connection with the spleen; and as yet, according to your correspondent, no one knows the functions of the spleen, so that experiments on dogs have not discovered the use of the spleen.—*Dr. Epps, Daily News, January 13th, 1864.*

(181.)—Dr. Powell had, however, so far as he could remember, in his experiments on a dog obtained results similar to those of Dr. G. Johnson and others. He wished, however, to draw attention to the fact that the same phenomena were not observed in experiments upon different animals. In a series of experiments which he had commenced last summer, he had found that in the cat, instead of closure, powerful abduction of the cords was obtained on galvanising the recurrences; and he thought it very important that we should ascertain which animal man resembled as regards the innervation of the larynx, in order to understand better the mechanism of nervous dyspnoea in aneurism. If man resembled the dog or the rabbit in this respect he might have spasm of the glottis as the result of irritation of the recurrent nerve; but if he resembled the cat, the so-called spasmodic dyspnoea, when of purely nervous production, was really due to paralysis. And it was even possible that a ready means of relief might be found in galvanism, for nothing could be more striking than the instant relief afforded to the dyspnoea in the cat when the divided recurrences were galvanised.

Dr. Johnson, in reply, said there must be some fallacy in Dr. Reid's experiment quoted by Dr. Powell; it did not agree with Dr. Powell's results on the cat, which were in accord with those of Dr. Rutherford.—*Dr. G. Johnson, British Medical Journal, No. 729, pp. 791-2.*

(182.)—That the experiments of Fritsch, Hitzig, Ferrier, Nothnagel, and others are interesting, and in some sense instructive, is indisputable; but that they will contribute in any degree to render more certain the inferences of phrenology—that they will, even to the

smallest amount, form the groundwork for a more accurate knowledge of the physiology and pathology of the cephalic mass—is, I very much fear, neither more nor less than a delusion and a snare; whilst the experiments of the Swiss physiologist, Gudden, showing, in plain language, that organs that are not or cannot be used are sure to waste; that abolishment of one sense is followed, as a rule, by increased activity and acuteness of some other sense, are worse than useless, inasmuch as they prove nothing but what has already been well understood, and what any reader of the *Bridgewater Treatise on Adaptation and Compensation* could have explained without having recourse to the vivisection practised by Gudden.—*Dr. Day, Lancet, No. 2,682, p. 120.*

(183.)—Experiments which have been made on the physiological action of codeia tend to show various results. These in some measure are due to the experiments having been conducted on animals of different species, genera and classes. Some, and those most reliable, have been on man; and, as it is for human benefit that we prosecute these researches, it would be better if further secondary experiments were always so conducted.

Dr. Stocker injected some cats subcutaneously with hydrochlorate of codeia. In each instance the pupils were dilated; cerebral congestion was present, as determined by ophthalmoscopic examination; there was much reflex excitability; in one case, epileptic convulsions; salivation and purging occurred in two cases; there was no vomiting. The dose used was about a grain and a half.

In the case quoted there are several points of disagreement with these results, which are probably due to the difference in the animals—one a man, the other a cat. Thus the cat's pupils were dilated, the man's contracted, and, while there was no increase of alvine or salivary secretions in the man, two of the cats were salivated and purged.—*Mr. Moore, British Medical Journal, No. 696, pp. 576 and 577.* [See also *Langley, No. 45; Harley, No. 97; Marat, Thorowgood, and Lancet, No. 126; Yeo, No. 172; and Reynolds, No. 175.*]

(184.)—The next source of evidence of which I must speak is a most important one, a strangely fascinating, and yet too often a most repulsive one:—I mean experiments on the lower animals. This source of evidence has been at all times largely drawn upon; sometimes, I fear we must admit, shamefully abused. There can be no doubt that physiology is vastly indebted to this means of investigation for some of its most brilliant results—it would betray utter ignorance of the subject to assert the contrary—results, however, which, in some instances, might perhaps have been otherwise,

although more slowly, yet as surely arrived at. The fruits of this method of investigation are disproportionate to its relative value, because it has been far more extensively tasked than any other. The reason of this is obvious. Experiments yield direct and immediate results; and this method of research requires less knowledge, labour, and patience than some others, and is more readily and, in many respects, conveniently carried on.

You will see at once that the value of this kind of evidence rests wholly upon that on which we have previously insisted—the relation of animals in their structure and functions to each other and to man. We virtually assume this, of course, whenever we appeal to them either for observation or for experiment. But here a most important question arises. Animals, of course, though related to man in kind, yet differ from him in degree. Then to what extent do they differ? How closely are they related? How far can conclusions drawn from experiments on animals become applicable to man? This is a delicate question to answer, and one which is too often lost sight of altogether. If I had to give a concise answer to the inquiry, I should say, as above, that the difference was one of degree merely, not of kind.

Thus conclusions drawn from experiments upon the muscles of the higher animals, as those with electricity, may fairly be applied to the human subject: seeing that in their anatomy and physiology these structures bear the closest resemblance in the two cases.

On the contrary, conclusions drawn from experiments upon the various fluids poured into the alimentary canal—upon gastric juice, bile, saliva, pancreatic juice, whether concerning their quality or quantity—in animals, must be applied with the utmost caution to man, for of course much must depend on the nature of the food and other circumstances associated with it.

You cannot assume common conditions for carnivorous, and herbivorous, and omnivorous creatures.

Too often experiments are only coarse and clumsy caricatures of nature. Many experiments are merely very rough and imperfect attempts to produce, artificially, conditions which are already naturally, and hence most perfectly prepared. I allude here more particularly to such experiments as many of those which have been performed upon the nerve-centres; the brain, for example. It has oftentimes been attempted to determine the functions of different portions of the Gordian knot of physiology in a right royal fashion, by cutting it through, and then endeavouring to settle the question by observing the consequences.

But who that knows anything of the anatomy of this organ, or of physiology in general, would be inclined to pin his faith to such a

result? Imagine the disturbance produced by such operation, especially upon the functions of the organ in question, even when most skilfully performed. And here I am bound to say that some physiologists, from natural dexterity and much practice, do manage many of these experiments, delicate and intricate ones, with an amount of skill and ingenuity that would scarcely be deemed possible by anyone who had not witnessed them. Yet, in spite of this, I contend that in many cases the result is negatived by the operation itself; whereas no fatal error of this kind can arise from careful observation of the faculties and habits of animals in comparison with the development of their brains. To trace the evolution of particular faculties *pari passu* with different portions of this organ is unquestionably surer, safer, and in every respect more philosophical. Hence it has been happily expressed, that in the various structures and functions of the lower animals we have, to our use already, experiments prepared for us by the hand of nature. And surely it is more reasonable, and becomes us better, to seek information from her voluntary replies—and she is never silent when earnestly questioned—than to attempt by violence to wring them from her. The evidence of confession evoked by torture has always been most justly regarded as equivocal.)

But do not understand me to urge these objections against all experiments. I cannot confess to any sympathy with those who express unmitigated disgust at what is called vivisection. Such prejudices are founded upon shallow arguments, and form no evidence of a rightly constituted mind. Pain and torture by no means form a necessary part of all vivisections. In fact, there are but few experiments, and those only of a certain kind, which cannot with great advantage be performed under chloroform. And this application of chloroform is in no sense amongst the meanest of its uses. While it entirely removes all objection to vivisection, it renders the performance of many experiments, otherwise impracticable, easy and satisfactory. The heart, for instance, may be exposed in a living warm-blooded animal under chloroform, and its movements watched and submitted to experiment. Here is not only the objection to vivisection, on the score of the animal's suffering or consciousness, completely set aside, but our observations are not embarrassed, or their value lessened by its struggles or distress. In a word, the employment of chloroform meets every reasonable argument which can be urged against vivisection.—*Dr. Savory, Lancet*, No. 2,001, p. 30.

(185.)—We [*Times*] have great pleasure in publishing the following important protest:—

We, the Court of Examiners for Scotland of the Royal College of Veterinary Surgeons, desire to express our opinion that the per-

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(185.)—We [*Times*] have great pleasure in publishing the following important protest:—

We, the Court of Examiners for Scotland of the Royal College of Veterinary Surgeons, desire to express our opinion that the per-

formance of operations on living animals is altogether unnecessary and useless for the purpose of causation.

JAMES SYME, M.D., *Chairman*, and six others.

I fully concur in the above.

WILLIAM WILKINSON,

Principal Veterinary Surgeon to the Forces.

[The following letter was written in reply to Dr. Bennett, who opposed Dr. Syme's appointment as President of the Edinburgh Medical Council, on the ground that Mr. Syme disapproved of modern physiological teaching, "having on one occasion heard a rabbit squeak in the hands of Brown-Séquard, when he became so affected as to desire a complete suppression of physiological experiments on animals." Also because he had stated publicly "there cannot be, I think, any procedure more shocking than propping up a poor creature suffering from disease of the lungs, and hammering his chest for the recognition of diagnostic sounds as an academical exercise":—

Sir,—In reply to Dr. Bennett, it is hardly necessary to say that I never characterised "microscopes and ophthalmoscopes" as physiological toys. What I have said is, that this title seemed applicable, not to these most valuable instruments, but to those which, instead of promoting the progress of science, simply demonstrated certain ascertained physiological facts. As to the dissection of living animals, I have always expressed the opinion that it was warranted for scientific investigation, but not for merely exhibiting the results obtained. —JAMES SYME, January 30th, 1869.—*Lancet*, No. 2,371, p. 194.]

(186.)—Sharing in the feeling against vivisection, which is strong and general, we wish to take part in the movement against it, and on the following grounds:—

The practice is supported on the plea that it is essential to sound physiological teaching; and, secondly, that it is a necessary instrument of original research,—that biology, as an experimental science, requires a large use of experiment.

The first plea we reject as untenable. We do not consider that the student, medical or strictly scientific, requires the aid of vivisection for attaining the mastery of the science which he needs for his subsequent course.

As an instrument of original research vivisection stands on a somewhat different footing. It has unquestionably been useful in the past. We think that, under most careful restrictions, it may on rare occasions be needed in the future. But the results already attained in biology ought to render its employment more and more infrequent. And the most important branch of inquiry, that which relates to the cerebral functions, is the one in which we think other methods most

applicable, the method of vivisection most uncertain and most unfruitful.

On several grounds we are still more opposed to its employment at all as an instrument of teaching, and call for its most guarded employment by the scientific inquirer. We object to any such addition to the hardening process of intellectual training as is involved in the constant familiarity with animal suffering, and we consider this objection to hold very forcibly even when the student is no longer a mere youth.

We object to it as an abuse of our power over the animals, whose chief, man has gradually risen to be—a power which is limited both by our own moral nature and by the kindred nature of these animals. We lay down no absolute doctrine; some animal races are inconsistent with man—the poisonous snakes for instance—and these he exterminates, but he might not, if he could, torture them. All others, whether indifferent or useful, he should protect from needless injury, and use with merciful care, inflicting no avoidable suffering where some suffering is inevitable. This leads us to the limited use of vivisection. We think that man, who takes animal life so freely for his food, may occasionally inflict suffering for the purpose of clearing up his scientific knowledge; that in some cases when a definite problem is before us—not therefore as a means of chance discoveries—if on careful investigation it appear that such problem, given its sufficient importance, would probably be solved at the cost of some animal suffering, it would be legitimate to exact that suffering. These conditions would evidently limit the practice almost indefinitely.

RICHARD CONGREVE, M.R.C.P.

J. H. BRIDGES, M.B., F.R.C.P.

—*Fortnightly Review*, March, 1875.

(187.)—The present outcry against vivisection has had its origin, we fear, in an abuse of physiological demonstrations. We abhor the man who is guilty of cruelty towards the inferior animals; and he who would unnecessarily torture them even in the ends of science is only worthy of contempt. At the same time we believe that public opinion has been unduly excited on this question, and that unless restrained it is likely to lead to the adoption of measures which are sure to fail through their stringency. Physiologists have still to solve some problems of life and organisation for the benefit of the living and the diseased, and if the Legislature can step in and provide that investigations shall be undertaken only by experts and on narcotised animals, public opinion ought to be satisfied.

In this institution experimental physiological teaching has never been encouraged, although it has doubtless witnessed many experiments which have been undertaken to settle important points connected both with physiology and pathology. even from the time

when John Hunter was the guiding star of its medical committee onwards to the present. But we repeat, that its pathway has not been stained by the blood of animals unnecessarily sacrificed for practical expositions. Its students have become eminent without such a help as this.

We hold also that these experiments are not absolutely required in medical or veterinary institutions. If schools existed for the exclusive study of physiology, a warrant for daily experimental teaching might, under prescribed regulations, be possibly found; but the establishment and constitution of medical schools have a far different object. Students throng their wards for the purpose of obtaining a general knowledge of the science and practice of medicine and surgery, and perhaps scarcely one in a thousand enters on his studies for the purpose of giving especial attention to experimental demonstrations with a view of ultimately becoming a physiologist *per se*. Are, then, we may ask, experiments on narcotised animals to be oft repeated that all may *see*, when the smallest possible proportion only are specially instructed thereby? Are we to risk the blunting of the finer feelings of human nature in hundreds of young men for the advantage of the twos and threes? No! emphatically no. Ever bear in mind, gentlemen, that the study of the ordinary functions of the living body, and also of the nature of disease—ordinary as well as special—with an analysis of symptoms, and searching post-mortem examinations in relation to the progress of fatal cases of disease, will, with the expositions of such a teacher as you possess, make you really good and practical physiologists.—*Professor Simond's address at the opening of the Winter Session, Royal Veterinary College, October, 1875.*

(188.)—On the motion of Professor Sir William Thomson, at a meeting of the Society for the Prevention of Cruelty to Animals held in Glasgow recently, it was agreed to petition Parliament in favour of a Bill to enforce proper restrictions on the practice of vivisection. Sir William Thomson said there was a tendency to have vivisection repeated unnecessarily. It was far from his intention to join in any movement which would unduly restrict men of science in whatever operations might be necessary for advancing knowledge; but he considered that the repetition of cruel experiments on the lower animals, merely for the purpose of showing students what had been done, was altogether unnecessary.

Professor Nichol, who seconded the motion, said that when it was necessary to resort to that practice, precaution should be taken to reduce the pain to a minimum, and that when the experiment was over, the animals should be put to death.—*British Medical Journal*, No. 744 pp. 454-5.

(189.)—Sir William Thomson said : He was very far from wishing to join in any movement that would unduly restrict men of science in whatever operations were required for advancing knowledge ; but he held that the repetition of cruel experiments on the lower animals, merely for the purpose of illustrating to the students what had been done, were altogether unnecessary. (Applause.) He was well acquainted with what was usual and what practice seemed to point out to be useful in the way of experimental demonstration to students of the truths of science. In physical science it was held to be desirable to repeat experiments, even when the repetition did not lead to any new knowledge, but he held that that was a mere superfluity. He contended that a teacher might teach the truths of science without repeating experiments, and that repetition in the way of lecture demonstration was merely an *agrément*, as it were, to the students, and was not at all necessary, in respect to the intellectual position, to the full appreciation of the truth to be demonstrated. When an experiment involved pain to any sentient creature it ought not to be made. (Hear, hear.) As Professor Nicol said, there was an education in vice as there was an education in virtue. What could be a more complete education in that most devilish of all vices—wanton cruelty—than repetition, merely as an *agrément* to the students, of some experiment involving torture to a sentient creature? Without going into details as to the particular points which should be pressed in the memorial to Parliament with a view to imposing restrictions on vivisection, he would suggest that popular lecture experiments or laboratory exercises, merely for the sake of instruction or illustration to students, ought to be very carefully guarded against, and the possibility of this generating into an education in cruelty.—*Glasgow Herald*, March 30th, 1875.

(190.)—Professor Nichol said : It might be contended that this question should not be dealt with by laymen, but should be left entirely to medical men. In answer to such an argument he would say that personally he had been induced to form and emboldened to express an opinion as to the necessity of some steps being taken with reference to vivisection on the testimony of medical men. Extracts from medical journals showed beyond a doubt that this practice of vivisection, which in many instances entailed far more exquisite suffering than any form of slaughtering, had been practised to a large extent by untrained students, and without certain precautions which tended greatly to restrict the cruelty, and that it had been largely practised for the purpose of making lectures more vivid. There were a great many things that might make lectures more vivid, but which of course were utterly out of the question. Supposing a lecturer advertised that he meant to deal with the question of the Inquisition, and that certain persons would be tortured in presence of the audience, he would doubtless attract a very large

gathering, but the proposal would of course be condemned as illegal. This was obviously an extreme case, involving torture to a human being, but the difference was one of degree, not of kind. (Hear, hear.) We were not more entitled to torture an animal than a human being without cause. The difference was this, that we were in no circumstances entitled to torture a human being, whereas it might be permissible in reference to an animal, but these circumstances were very exceptional. As to the suggestion that this was a mere sentimental outcry, he would remind them of the list of names which Sir William Thomson had read, in which the name of Thomas Carlyle occurred, than whom a more unsentimental man never existed. After making one or two practical suggestions Professor Nichol said: 1st, That no person should be allowed to perform operations on living animals except educated medical men, in positions of authority, who might be trusted to perform them only on strong and sound reasons; 2nd, That they should only be performed for the purpose of discovery, not of illustration, which latter, unfortunately, was apt, in some cases, to become the purpose of amusement; and, 3rd, That when an operation was necessary, every precaution should be taken to reduce the pain to a minimum; that was to say, anæsthetics should be employed, and when the animal had been liberated, and the experiment over, it should be put to death. Many persons would be surprised to learn that dogs and cats had been allowed to crawl about for weeks on the table of a dissecting-room after they had been mutilated, waiting until required for further experiment.—*Glasgow Herald*, March 30th, 1875.

(191.)—There are certain conditions the fulfilment of which is indispensable to the proper and rightful performance of experiments on animals. These conditions are capable of plain definition. The principle involved in them is this,—That all destruction of animal life, all pain inflicted, which are unnecessary for the elucidation of the legitimate objects of inquiry are unjustifiable.

First, then, it is required of the experimenter himself, that he be equal to the performance of the task which he has undertaken; that he be a skilled anatomist and physiologist, capable of reading and appreciating all the facts which experiments may offer to his observation; that he has made himself master of all the known facts affecting the matter which he is about to investigate; that he has in a word duly, I might say solemnly, prepared himself to perform the business he has taken in hand.

The proper and only object of all justifiable experiments on animals is,—To determine unknown facts in physiology, pathology and therapeutics whereby medical knowledge may be directly or indirectly advanced. When therefore any fact of this kind has been once determined and positively acquired to science all repetitions of experi-

ments for its further demonstration are unnecessary, and therefore unjustifiable.

All experiments, therefore, performed before students, in classes or otherwise, for the purpose of demonstrating known facts in physiology or therapeutics are unjustifiable. And they are especially unjustifiable, because they are performed before those who, being mere students, are incapable of fully appreciating their value and meaning. And they are needless and cruel; needless, because they demonstrate that which is already acquired to science; and especially cruel, if admitted as a recognised part of students' instruction, their constant and continued repetition through all time would be required. [I perhaps need hardly say that courses of experimental physiology are nowhere given in this country; and that these remarks consequently apply only to those schools in France and elsewhere where demonstrations of this kind are delivered.]

A consideration of the conditions here proposed as requisite for the rightful performance of experiments on living animals shows,—That experiments in this wise must ever be very limited, because those persons who are fitted for the due performance of them are of necessity few in number, and that in proportion as new facts are added by them to our knowledge, the experiments must diminish in number.

Moreover, humanity has a right to demand of the experimenter that he subject the animal experimented on to the influence of chloroform or of some other anæsthetic, in every case in which the exhibition thereof is practicable, or in which the elucidation of the subject of inquiry is not interfered with by the action of the anæsthetic. And, it may be added, that there are very few experiments in which the administration of an anæsthetic to the animal experimented on is not practicable during at all events some, and probably the most painful part of the experiment. The physiological experiments in which it is required that sensibility should exist on the part of the animal, at least during the whole period of the experiment, are exceedingly few.—*Dr. Markham.*

I entirely concur in the unanimous conclusion of the Essayists in denouncing the stupid and atrocious system which assumes it to be necessary for the veterinarian tyro to draw the red-hot tool along the skin of a living horse in order to enable him to apply it properly in a case requiring firing.

I equally reprobate the performance of experiments on living animals to show to students what such experiments have taught the master; whilst the arguments for learning to experiment by repeating experiments on living animals, are as futile as those for so learning to operate chirurgically.

The conditions under which vivisectional and other methods of experimenting on living animals, are requisite, justifiable, and even praiseworthy, are, in my humble opinion, those defined in Essay E. [Dr. Markham's]—*Professor Owen.*

(192.)—How few facts of immediate considerable value to our race have of late years been extorted from the dreadful sufferings of dumb animals, the cold blooded cruelties now more and more practised under the authority of science!

The horrors of vivisection have supplanted the solemnity, the thrilling fascination of the old unetherised operation upon the human sufferer. Their recorded phenomena, stored away by the physiological inquisitor on dusty shelves, as mostly as of little present value to man as the knowledge of a new comet or of a tungstate of Zirconium! perhaps to be confuted the next year; perhaps to remain as fixed truth of immediate value.—contemptible, compared with the price paid for it in agony and torture. For every inch cut by one of these experimenters in the quivering tissues of the helpless dog or rabbit or guinea-pig, let him insert a lancet, one-eighth of an inch into his own skin, and for every inch more he cuts let him advance the lancet another eighth of an inch, and whenever he seizes, with ragged forceps, a nerve or spinal-marrow, the seat of all that is concentrated and exquisite in agony, or literally tears out nerves by their roots, let him cut only one-eighth of an inch further, and he may have some faint suggestion of the atrocity he is perpetrating, when the guinea-pig shrieks, the poor dog yells, the noble horse groans and strains—the heartless vivisector perhaps resenting the struggle which annoys him.

My heart sickens as I recall the spectacle at Alfort, in former times, of a wretched horse, one of many hundreds, broken with age and disease resulting from life-long and honest devotion to man's service, bound upon the floor, his skin scored with a knife like a gridiron, his eyes and ears cut out, his teeth pulled, his arteries laid bare, his nerves exposed and pinched and severed, his hoofs pared to the quick, and every conceivable and fiendish torture inflicted upon him, while he groaned and gasped, his life carefully preserved under this continued and hellish torment, from early morning until afternoon, for the purpose, as was avowed, of familiarising the pupil with the motions of the animal. This was surgical vivisection on a little larger scale, and transcends but little the scenes in a physiological laboratory. I have heard it said that "somebody must do this." I say it is needless. Nobody should do it. Watch the students at a vivisection. It is the blood and suffering, not the science, that rivets their breathless attention. If hospital service makes young students less tender of suffering, vivisection deadens their humanity, and begets indifference to it.

In experiments upon the nervous system of the living animals whose sensibility must be kept alive, not benumbed by the blessed influence of anæsthesia, a prodigal waste of suffering results from the difficulty of assigning to each experiment its precise and proximate effect. The rumpled feathers of a pigeon deprived of his cerebellum may indicate, not so much a specific action of the cerebellum on the skin,

as the more probable fact that the poor bird feels sick. The rotatory phenomena, once considered so curious a result of the removal of a cerebral lobe, were afterwards suspected to proceed from the struggles of the victim with his remaining undamaged and unpalsied side. Who can say whether the guinea-pig, the pinching of whose carefully sensitised neck throws him into convulsions, attains this blessed momentary respite of insensibility by an unexplained special machinery of the nervous currents, or a sensibility too exquisitely acute for animal endurance? Better that I or my friend should die than protract existence through accumulated years of torture upon animals whose exquisite suffering we cannot fail to infer, even though they may have neither voice nor feature to express it.

If a skilfully constructed hypothesis could be elaborated up to the point of experimental test by the most accomplished and successful philosopher, and if then a single experiment, though cruel, would for ever settle it, we might reluctantly admit that it was justified. But the instincts of our common humanity indignantly remonstrate against the testing of clumsy or unimportant hypotheses by prodigal experimentation, or making the torture of animals an exhibition to enlarge a medical school, or for the entertainment of students, not one in fifty of whom can turn it to any profitable account. The limit of such physiological experiment, in its utmost latitude, should be to establish truth in the hands of a skilful experimenter, with the greatest economy of suffering, and not to demonstrate it to ignorant classes and encourage them to repeat it.

The re-action which follows every excess will, in time, bear indignantly upon this. Until then it is dreadful to think how many poor animals will be subjected to excruciating agony, as one medical college after another becomes penetrated with the idea that vivisection is a part of a modern teaching, and that, to hold way with other institutions, they too must have their vivisector, their mutilated dogs, their guinea-pigs, their rabbits, their chamber of torture and of horrors, to advertise as a laboratory.—*Dr. Bigelow, Professor of Surgery at Harvard University, being extracts from the Annual Address read before the Massachusetts Medical Society, 1871.*

(193.)—For ourselves we are wholly of opinion that experimental physiology should be distinctly separated from those experimental sciences which deal only with inanimate matter. It is not work that ought to be followed up by the young student like a course of practical chemistry, or botany, or mechanics. The experiments are means of discovery; they are not means to the continued exercise of learning. Least of all are they in any degree training experiments; and though it may be necessary now and then for a master to demonstrate them for the sake of truth, it is the master only that should demonstrate, and even he with all the caution and all the

humanity he can reasonably command.—*British and Foreign Medico-Chirurgical Review*, No. 110, p. 283.

(194.)—The meeting of Convocation of the University of London has this year been remarkable for two things. The first was the passing of a resolution recommending the admission of women to degrees. The second was the refusal to entertain a resolution requesting the Senate not to allow painful experiments on living animals to be carried on at the Brown Institution.

This latter resolution was a direct blow aimed at vivisection, and was moved in a most able speech by Mr. Hutton, M.A., a gentleman of great influence. Only sixteen voted with him and fifty-nine against,—a result, we believe, partly due to the Chairman reading a remarkable letter from Dr. B. Sanderson, the learned Superintendent of the Brown Institution, who stated to our amazement that the laboratory was not used for physiological inquiries, only for investigations into the nature and treatment of disease. It was necessary, he added, to perform small experiments, such as the abstraction of a little blood, &c. We have expressed surprise at this statement. Can it be possible that Dr. Sanderson is ignorant of the fact that physiological teaching is carried on at the Brown Institution, and that such teaching is based on physiological experiments of the most painful kind? The controversy on vivisection has been best handled in the columns of the *Spectator*, and most men have come to the conclusion that for proper purposes, within certain limits, vivisection is justifiable, but that anæsthetics should be employed whenever possible. Indeed, it has been urged by many that pain is very seldom inflicted, because the use of anæsthetics is universal. Now this we deny. In several places in London most painful experiments have been recently performed without producing anæsthesia, and the Brown Institution is one of them. Indeed, after Dr. Sanderson's letter, we think it right to assert that, whether he knows it or not, the use of anæsthetics is the exception, not the rule, in the institution.

We take the liberty too of describing two of the experiments lately done, and we suppose frequently done, for teaching purposes, in order that Mr. Hutton may at least have a faint idea of the facts he might easily collect.

First experiment.—Take a lively frog and tie his four legs to a square board, stretching the legs so that he cannot writhe; crucify him in fact. Then make two cuts, one on each side of the sternum. Another cut across the breast-bone is the next step. Then cut through the bone, and take it right out.

The next step in the vivisection is to make a cut the whole length of the abdomen, so as to open the cavity. The end of the heart, which is now visible beating, is to be cut off. Through the opening thus made in the heart push a cannula right up to the

bulbos aortæ. Now open the abdominal vein. All this in preparation for the experiment itself, which is thus proceeded with: Inject warm distilled water into the cannula until the whole of the blood has been washed out of the frog, when he will appear to be dead, and swollen up to about four times his original bulk. The injection takes some ten minutes. After the water an injection of silver nitrate is to be made.

Second experiment.—Press your finger under a frog's mouth until the eyeball protrudes. Then pull down his nictitating membrane with a pair of forceps. Next, scrape off with a sharp knife the three layers of anterior epithelium. Now let him rest ten minutes. You may then observe his actions: he pokes his head down between his fore-legs, jumps, or turns over, wriggles, and otherwise acts in a strange manner.

Having watched this, take him again and push out the eyeball once more. Again hold down the protecting membrane, and rub a stick of solid lunar caustic all over the eye until the aqueous humour of the anterior chamber shows a precipitate. Now release your frog, and his actions will be similar to those previously watched, but more intense; in fact he plays such fantastic tricks as few could look upon without that blunting of sensibility on which so many have dilated. It is necessary for him to be left for ten minutes, after which his head is cut off with a pair of scissors, and so his suffering ended.

The object is to demonstrate the structure of the cornea by staining the intercellular substance. We do not propose to assert that these experiments are useless, but we do say that they could be just as easily performed under the influence of anæsthetics. If Mr. Hutton has any doubt that they are done, we have none. We beg to say that they have been done since his speech in Convocation, since Dr. Sanderson's letter, and done too without chloroform or any other anæsthetic.

We have no desire to put an end to scientific investigation, but needless cruelty seems to require curbing, and Mr. Hutton is fighting in a good cause. If he wants details of other experiments he has only to read the *Handbook* edited by Dr. Sanderson himself, and be informed that that work is used as a text book at the Brown Institution. So it is in most of our medical schools. In the majority of the latter, chloroform is used.—*The Doctor*, June 1st, 1874, p. 101.

In reference to the above, Dr. B. Sanderson wrote to *The Doctor*, July 1st, 1874, as follows:—

“Sir,—My attention has to-day been drawn to an article in your journal of June 1st, headed ‘Vivisection,’ in which you comment on the management of the laboratory of the Brown Institution. All that is necessary for me to say with reference to the article is, first, that so far as relates to the laboratory of the Brown Institution, the assertions

contained in it are untrue ; and secondly, that the statements with reference to this subject which were embodied in the letter to the Chairman of Convocation of the University of London, to which you refer, are perfectly correct.

“ Your obedient Servant,

“ The Brown Institution,
June 26th, 1874.”

J. BURDON-SANDERSON.

[We are amazed at this letter, which we invite the writer to reconcile with the facts. The assertions contained in our article are true. We did not comment on the management of the “laboratory.” Does Dr. Sanderson mean to deny that the experiments we described in *The Doctor* for June 1st have been performed in the Brown Institution? We deliberately repeat that within the walls of the Institution such experiments have been frequently performed.—ED. *Doctor*.]

(195.)—We do not condemn vivisection *in toto*. We consider that in the hands of men like John Hunter, Sir Charles Bell, Marshall Hall, Brown-Séquard, Claude Bernard, and others it is perfectly justifiable, for it has added and will add to our means of alleviating human suffering ; but we hold also, that even in the hands of such accomplished men, no unnecessary torture should be inflicted. For instance, the boon of chloroform ought not to be withheld on the ground of either trouble or expense, and we believe that every rightly-constituted mind would be glad, whenever possible, to avail himself of this agent.

Researches carried on by qualified men differ altogether from vivisection for the purpose of demonstrating to students well-known facts, still more from the performance of operations on living animals by students themselves. The suggestion that every student should verify the ordinary facts of physiology is monstrous.

No possible good could come from the demonstration at Norwich ; all who wished to inquire about the facts could understand published descriptions. The experiments, therefore, merely pandered to a love of sensation.

The experiment we described last year, as performed in the Brown Institution, has since been repeated there and elsewhere, and it will be remembered by our readers that we inserted a letter from Dr. Sanderson impugning our statements. To his letter we appended an expression of surprise, and asked him whether he could deny that such experiments were performed in the Brown Institution. To that challenge we have never received a reply, and we beg publicly to announce that the cruelties to which we have adverted are regularly practised in the Brown Institution, and that for the purpose of teaching. In fact, that institution may be looked upon as the centre of vivisection, whence its pupils go forth to practise

elsewhere. What would the benevolent Mr. Brown have thought had he foreseen to what his legacy for a hospital for animals would lead?—*The Doctor*, February, 1875. —————

(196.)—Will you grant me the space in your journal for a few words called forth by Mr. Lewes's letter in your number of December 4th, on "Sensation in the Spinal Cord"?

In that letter the writer describes some experiments on frogs of such excessive cruelty that I cannot refrain from entering a protest against the principle which justifies such actions.

The right to perform such actions as vivisection, &c. in the cause of science has often before been questioned; but the present case, a case in which the infliction of pain is not an unavoidable attendant on the experiments, but the very essence or object of them, and the slowness and prolongation of agony a necessary part, stirs and revolts the whole mind, and brings the question again prominently to the front.

The question then is, are either the possible or probable benefits to a portion of mankind, or the advancement of science for its own sake, sufficient reasons for the infliction of intense suffering on our fellow-animals? Of course much may be urged in favour of vivisection. It may be said that without its assistance science, and especially the science of medicine, could never have advanced to the point it has now reached; and mankind urges that the good of mankind is of such paramount importance that, that of all other animals must be subordinated to it unconditionally, and consequently that the smallest good to mankind balances the greatest evil to other animals.

To many this would be considered an amply sufficient reason for answering the question in the affirmative, but at least it should be remembered at what tremendous cost to one portion of creation these benefits to another portion are purchased.

As time and science advance it is becoming more recognised that other animals have their rights as well as men; and perhaps it may some day be found that the right which mankind assumes to himself of supremacy over his fellow-animals (including the right to inflict deliberate torture, for whatever purpose) is, after all, but the right of the strongest or most powerful.

It seems to me so shocking that such things should be written of and read with indifference, and without evoking one word of protest on the other side, that on this ground alone, *i.e.*, that the assumption of the right to inflict torture may not pass quite unchallenged, I venture to beg for the insertion of this letter.—X., December 8th.—Letter to *Nature*, No. 216, p. 121. —————

(197.)—As far as natural science goes the extra time which has to be expended in obtaining the results practically, is generally quite made up for by the accessory details introduced, which are many of

them omitted in written or verbal descriptions. Observation is a far more sound basis on which to start fresh work than the knowledge acquired from books alone. The student should, therefore, where nothing counter indicates, have the opportunity of repeating on his own account the experiments he reads of. In the case of practical physiology, however, another consideration has to be introduced.

Here the subjects of experiment are sentient beings, and the question comes to be, whether the advantages of the practical verification of fully described phenomena which involve pain are counterbalanced by the injustice done in the production of pain itself. We think not, and are therefore fully in favour of legislative restrictions on the powers of those who wish to employ living animals for the purpose of demonstration, even where anæsthetics are employed, because there is a tendency among those who are in the habit of repeating experiments to neglect those parts of them which are not absolutely necessary. But any measure which in any way impedes original work, as does the Bill before us, ought, in our opinion, to be strongly opposed.—*Nature*, August 13th, 1875.

(198.)—The *Gazetta Italiana di Milano* contains an essay of Professor Mantegazza on experiments carried on under his direction at the laboratory of experimental pathology of the University of Pavia. It will suffice to state that the experiments were intended to study the action of pain on digestion and nutrition. They were, as the Professor himself confesses, agonising to the animals subjected to them and distressing to the experimenters, and simply proved that loss of appetite, great weakness, and a peculiar imbibition of moisture were the result of the pain inflicted. It is added that no alteration of the spinal marrow could be detected after the agony had been protracted for one month. Very meagre results of unpardonable cruelty!—*Lancet*, No. 2,482, p. 415.

(199.)—We will state our belief however that there is too much of it everywhere, and that there are daily occurring practices in the schools of France which cry aloud, in the name both of honour and of humanity, for their immediate cessation. It has been indeed these pitiless procedures across the Channel that have immediately excited the present outcry. About two years ago our own Royal Society for the Prevention of Cruelty to Animals became possessed of the knowledge that it was still the practice in the schools of anatomy and physiology of France for the lecturers and demonstrators to tie down cats, dogs, rabbits, &c. before the class to perform upon them operations of great pain, and to pursue investigations accompanied by most terrible torture. This too for the purpose only of demonstrating certain facts which had been for long unhesitatingly admitted, and for giving a sort of meretricious or *ad captandum* air to a popular series of lectures.

It happened however that the editor of a journal, the office of which is situated close to the Ecole Pratique at Paris, complained to the police that both himself, the women in the adjacent Maternity Hospital, and in fact the whole neighbourhood, were constantly disturbed when the course of physiology was proceeding at the school by the howling and barking of dogs both night and day. The police were attentive, and visited the different rooms of the school, which "no longer containing subjects, are now filled with living dogs, which are shut up, probably without bread or water, ready for serving their turn at the lecture room." The police commissary, as the result of his visit, assured the editor "he would hear no more of it." "What," asked the latter, "are the dogs no longer to be kept in the dissecting rooms?" "Oh yes; but it appears that they have cut something in the dogs' necks which prevents them crying." The fact was, the poor animals were now subjected to the painful operation of dividing the laryngeal nerves as preliminary to the performance of other mutilations. And what were these dogs for? Simply for the vain repetition of clap-trap experiments by way of illustrating lectures for first-year students. These facts becoming known, the general public has at length interfered, and, as we think, with very great propriety. The entire picture of vivisectional illustration of ordinary lectures is to us personally repulsive in the extreme. Look, for instance, at the animal before us, stolen (to begin with) from his master, the poor creature hungry, tied up for days and nights pining for his home, is at length brought into the theatre. As his crouching and feeble form is strapped upon the table he licks the very hand that ties him. He struggles, but in vain, and uselessly expresses his fear and suffering, until a muzzle is buckled on his jaws to stifle every sound. The scalpel penetrates his quivering flesh, one effort only is now natural until his powers are exhausted—a vain instinctive resistance to the cruel form that stands over him, the impersonation of Magendie and others of his class. "I recall to mind," says Dr. Latour, "a poor dog, the roots of whose vertebral nerves Magendie desired to lay bare to demonstrate Bell's theory, which he claimed as his own. The dog, already mutilated and bleeding, twice escaped from under the implacable knife and threw his front paws around Magendie's neck, licking as if to soften his murderer and ask for mercy. Vivisectors may laugh, but I confess I was unable to endure that heartrending spectacle."

But the whole thing is too horrible to dwell upon. Heaven forbid that any description of students in this country should be witness of such deeds as these. We repudiate the whole of this class of procedure, science will refuse to recognise it as its offspring, and humanity shudders as it gazes on its face. And yet strangely and absurdly enough we have been quoted apparently as approving such heartless practices. In publishing the lectures of famous anatomists and physiologists we necessarily detailed the conclusions of the investigators

had arrived at, and indicated the means which they had pursued. But both conclusions and experiments were theirs, not ours, and both of which possibly might very little accord with what we really approved. It might with as much propriety be said we acquiesced in all that came out on the late trials of Russell *v.* Adams and Bromwich *v.* Waters, because we reported what the witnesses said. But for the present we must conclude; suffice it now to say, that we are of opinion that whilst under certain circumstances of a very particular character, every precaution being taken to lessen sensibility, animals yet endowed with life may be justifiably employed for the purpose of physiologic investigations by a few men of science in an almost sacred privacy and retirement, the practice of performing surgical operations on living animals, the custom of repeating the same experiments, and especially the illustrating of ordinary lectures through their means, should be at once and unhesitatingly abolished. *Lancet*, No. 2,086, pp. 224-5. _____

(200.)—For aught that we can learn Professor Schiff continues his vivisection as before, but with the difference that he anæsthetises his victims and prevents their cries.

We have too much faith in the English head and heart to be at all disturbed by the fear that the practices of the French and Italian veterinary schools and physiological laboratories will ever be acclimated among us.

But even in the halls of science "offences will come," and such as have been lately disclosed in foreign schools should be promptly protested against and put down. Here, and we would fain hope elsewhere, the conditions under which vivisection should be practised ought never to be other than these, that none but accredited teachers should be allowed to perform the operation, and that even the number of animals allowed in each school for the purpose should be restricted by law, while anæsthesia should invariably have been induced before the experimenter begins.—*Lancet*, No. 2,627, pp. 22 and 23.

[The *Spectator* of last week in adverting to the assertions of Professor Schiff, of Florence, and his assistant, M. Herzen, that their vivisections are always performed under chloroform and without pain to the subjects of them, declares that these statements must be accepted with the kind of qualification one would always extend to *ex parte* declarations. Our contemporary, writing on the authority of the positive statement received from the Florence Society for the Prevention of Cruelty to Animals, and signed by one of its inspectors, Signor Pontamari, declares that an unexpected visit to Professor Schiff's laboratory discovered the presence of a considerable number of living dogs, with open wounds in their throats, which the Professor confessed had been made by him to prevent the animals from howling

and disturbing the neighbourhood. Here is a direct challenge to Professor Schiff and M. Herzen which we think they are called upon to meet and explain.—*Lancet*, No. 2,656. p. 139.]

(201.)—We confess that we think Dr. Rutherford presses his principles too far when he urges that, teaching by demonstration being the most successful method, we are therefore always warranted in having recourse to it. Physiology and chemistry are both experimental sciences. The chemical lecturer can have no hesitation about employing any number of experiments, or of repeating them indefinitely to illustrate every step he takes; but we may fairly assume that the physiologist would be restrained by the thought that the materials with which he has to deal are not so much inert lifeless matter, but sentient living things. We hold therefore that it would be both unnecessary and cruel to demonstrate every physiological truth by experiment, or to repeat indefinitely the same experiment simply because, by such demonstrations, the lecturer could make his teaching more definite, precise, and valuable.—*Lancet*, No. 2,712, p. 286.

(202.)—When at any moment the practice overpassess the rigorous bounds of utility, when its object is no longer the pursuit of new solutions to scientific problems or the examination of hypotheses requiring a test, when vivisection is elevated into an art and this art becomes a matter of public demonstration, then it is degraded by the absence of a benevolent end and becomes a cruelty.—*Lancet*, No. 1,928, pp. 143-4.

(203.)—The defence of vivisection must, however, cease here. The moment that it surpasses the bounds of necessity, when it ceases to aim at the solution of problems in which humanity is interested, and becomes a new means of public demonstration having no benevolent end, then it is degraded to the level of a purposeless cruelty. The repetitive demonstration of known facts by public or private vivisections is an abuse which we deplore, and have more than once condemned.

The repeated torture of enfeebled animals for the instruction of students is not justified by any sufficient human interest.

Our English experimenters are in the habit of removing the subjects of their experiments from the sphere of sensation by narcotism, by ether, chloroform, vapour of puff-ball, or local congelation. This procedure should never be omitted on any fitting occasion. Guided by these rules, and tenderly observant of the considerations of humanity, our English physiologists and surgeons

have risen to the first rank in the world of science, to whose treasures they have made some of the noblest contributions. Their example may be proudly noted, and their course profitably imitated. —*Lancet*, No. 1,938, p. 396. —

(204.)—Animals have their rights every bit as much as a man has his; and although we would discourage expressions of false sentiment, we are glad to know that those experiments to which the strongest objections have been raised have not been made in this country.

It is for the purpose of instruction, however, that it becomes questionable whether, and to what extent, experiments of this kind should be performed. A chemical lecturer teaches well in proportion to the clearness with which he can demonstrate the correctness of his statements by experiment; and there is no doubt that it is the same with a lecturer on physiology. Some persons seem to regard the advance of knowledge as the whole duty of man, and they would, perhaps, consider experiments as justifiable in the one case as in the other. We cannot so regard it, for the simple and sufficient reason, as it seems to us, that the element of life and sensibility being present in the one case and not in the other carries a responsibility with it. Of course, if the physiological experimenter can render the animal as insensible as the reagents and retorts of the chemist, and if the animal's life can be taken whilst in this state of insensibility, or, at any rate, with no more pain than would be involved in sacrificing it for the meat market, the responsibility is vastly diminished. But we contend that in any case where certain phenomena are known to follow a given experiment, when the fact has been established by the separate and independent observation of many different persons, a lecturer is not justified in resorting to it for the purpose of mere demonstration where its performance involves suffering to the animal. —*Lancet*, No. 2,684, p. 204. —

(205.)—We hold, and have always held, that the indiscriminate repetition of experiments involving pain to animals for no purpose than to illustrate to a class of students facts already well known and established, is quite unjustifiable. —*Lancet*, No. 2,677, p. 877.

(206.)—We find learned physicians and anatomists and eminent veterinary surgeons uplifting their voices and wielding their pens in opposition to the gross abuse of vivisection. Independently of its indisputable barbarity, one great objection to the practice is that the operator is likely to be misled by the results obtained. The same operations frequently elicit the most conflicting phenomena. A distinguished medical man, Dr. Roche, who does not absolutely

oppose all experiments on living animals, has vigorously combatted those from which torture is inseparable. He urges that the dissection of a live animal, the pitiless cutting into its flesh and shedding its blood, exciting it to the utmost fury or plunging it into the stupidity of profound anguish and terror, is a strange means of ascertaining in what manner the peaceful and regular functions of a man in perfect cheerful health are exercised. Physically, as well as morally, it is a fact that no reliance can be placed on the replies to questions addressed to one under the torture. With very rare exceptions we have it on high medical authority such experiments lead to fallacious results, fill the student's mind with doubts and uncertainties, and are far better left alone. This is the case even when the operator possesses sufficient knowledge to profit to the utmost by his cruel practices, productive of indescribable sufferings to the helpless creatures subjected to them. "It is for them," Dr. Fée has written, "a slow and despairing death, so much the more horrible that they cannot lose consciousness. Men faint away, an animal never faints, but expires in torments. Certainly under skilful hands, sparing of the blood of animated beings, such deaths have borne fruits. But how many animals, immolated by the ignorant, have displayed their palpitating entrails to eyes that could not see, and to intelligence that could not understand?"

It has been urged that, if such experiments are to be made, it should be in the silence and retirement of the laboratory, and not in the dissecting school, before a crowd of spectators as has been and is too frequently the case. By witnessing such cruelties the young and inexperienced become inured and tempted to practise them without profit to themselves, and at double expense of suffering to the victims. It is related that when Dr. Ségalas was giving courses of physiology, the students (most of whom knew but little of human anatomy, while probably not one of them had a notion of comparative anatomy) used to steal dogs and entice cats into their lodgings and repeat upon them the experiments they had witnessed the day before.

An able medical writer, Dr. Bossu, has quite recently denounced the evil effects produced upon young men by familiarizing them with such cruelties. He declares that among the students who follow the lectures of the faculty there are many who cannot witness with impunity the shocking sight of an animal deprived of life slowly, methodically, and in the most cruel sufferings. Such an ignoble spectacle is calculated to prevent the generous sentiments of youth and it is important that it should disappear. "If such things continue," adds the editor of the *Medical Bee*, "future physicians will bear the impress of a cold implacable insensibility a hundred times put to the test and consequently very ill adapted to render them sympathetic to their patients. Let none deceive themselves: to accustom a man to shed blood, and to render him insensible to the sufferings of animals, is to degrade and brutalize him."

The worthy and humane Doctor concludes by advising the students to protest against such sanguinary experiments by absenting themselves from the physiological course whenever they are to occur, or leave the hall when the mutilations are about to commence.—*The Times* (Paris Correspondent), August 8th, 1863.

(207.)—At the meeting of the British Association at Liverpool, in 1870, the Committee of Section D (Biology) was requested to draw up a Report for the following purpose :—"To consider from time to time whether any steps can be taken by them or by the Association, which will tend to reduce to its minimum the suffering entailed by legitimate physiological inquiries, or any which will have the effect of employing the influences of the Association in the discouragement of experiments which are not clearly legitimate on live animals." The following is the Report which was accordingly drawn up and published in the *Medical Times and Gazette*, Feb. 25, 1871 :—

Report of the Committee appointed at the Liverpool Meeting (held 1870) of the British Association to consider the subject of Physiological Experimentation.

(Edinburgh : Volume 1871 of *British Association Reports*, p. 144.)

- I.—No experiment which can be performed under the influence of an anæsthetic ought to be done without it.
- II.—No painful experiment is justifiable for the mere purpose of illustrating a law or fact already demonstrated ; in other words, experimentation without the employment of anæsthetics is not a fitting exhibition for teaching purposes.
- III.—Whenever for the investigation of new truth it is necessary to make a painful experiment, every effort should be made to ensure success in order that the suffering inflicted may not be wasted. For this reason, no painful experiment ought to be performed by an unskilled person with inefficient instruments and assistance, or in places not suitable to the purpose, that is to say, anywhere except in physiological and pathological laboratories, with proper regulations.
- IV.—In the scientific preparation for veterinary practice, operations ought not to be performed upon living animals for the mere purpose of obtaining greater operative dexterity.

Signed by M. A. LAWSON *Oxford.*

G. M. HUMPHREY, *Cambridge.*

J. H. BALFOUR *Edinburgh*

ARTHUR GAMGEE, *Edinburgh.*

WILLIAM FLOWER, *Royal College of Surgeons, London.*

J. BURDON SANDERSON, *London.*

GEORGE ROLLESTON, (*Secretary*), *Oxford.*

[*End of Mr. Colam's evidence.*]

EPITOME OF OTHER EVIDENCE AND REPORT.

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Replies from Medical Schools to questions asked by the Royal Commission.

- ST. BARTHOLOMEW'S.—Twenty or thirty frogs and about half as many dogs, cats and rabbits for original research and class demonstration. In the latter anæsthesia was always present.
- ST. THOMAS'S.—Three or four frogs for class demonstration without anæsthetics.
- GUY'S.—Forty to fifty dogs, rabbits, guinea-pigs, frogs and rats for class demonstration. About twenty for original research. All experiments for class purposes and almost invariably all for original research under anæsthetics.
- ST. GEORGE'S.—Five or six frogs for original research under curare but none for teaching purposes.
- LONDON HOSPITAL AND MEDICAL COLLEGE.—Two or three frogs, deprived of a drop of blood for teaching purposes.
- MIDDLESEX.—None last year. Previously perhaps for class demonstration, half-dozen frogs and rabbits, perfectly insensible.
- UNIVERSITY COLLEGE.—About a hundred frogs, six dogs, seven cats six rabbits and sometimes a few guinea-pigs for demonstration and research, all animals being in a state of anæsthesia.
- KING'S COLLEGE.—Two or three hundred frogs, newts, tritons, dityscus, rabbits, cats, dogs, guinea-pigs, for demonstration and original research, generally in a state of anæsthesia.
- WESTMINSTER.—About fifteen frogs for class demonstration, the majority being under curare.
- CHARING CROSS.—No account kept. Number small, but pigeons rabbits, frogs, newts, and water-beetles perhaps once a year, the smaller animals being curarised and the larger chloralised.
- ST. MARY'S.—None.
- QUEEN'S COLLEGE, BIRMINGHAM.—No return.
- BRISTOL MEDICAL SCHOOL.—Two or three rabbits drowned for class demonstration.
- UNIVERSITY OF CAMBRIDGE.—About two hundred dogs, cats, rabbits, tortoises, snakes, snails, frogs, toads, for original research and class demonstration, all dogs, cats and rabbits narcotised. Frogs generally, but always when used for teaching purposes.
- LEEDS SCHOOL OF MEDICINE.—None.

- LIVERPOOL ROYAL INFIRMARY SCHOOL OF MEDICINE.—About sixty rabbits, monkeys, tortoises and frogs for original research and class demonstration, in every instance deep anaesthesia present.
- OWEN'S COLLEGE.—No record of number kept. Rabbits, frogs, rats, guinea-pigs, dogs and cats for original research and teaching purposes. In the latter anaesthetics are used, and in all other cases except when death is rapid.
- OXFORD.—(Anatomical Department Museum.)—About six frogs for class demonstration and original research, anaesthetics being employed always when pain is likely to be caused.
- GENERAL HOSPITAL (Radcliffe Infirmary) and Sanitary Laboratory, Medical Department, in the Museum—None.
- EXETER COLLEGE LABORATORY.—About twenty frogs for scientific investigation and teaching purposes. Anaesthetics not used.
- MAGDALEN COLLEGE LABORATORY.—Exclusive of pithed frogs, ten to twenty rabbits, dogs, frogs and fish for research and class demonstration, nearly all anaesthetised, curare occasionally used.
- SHEFFIELD SCHOOL OF MEDICINE.—One frog and one rabbit for class demonstration, the rabbit being in a state of anaesthesia and the frog not subjected to pain.
- UNIVERSITY OF DURHAM.—Intend to perform experiments for scientific and teaching purposes on frogs, rabbits, dogs and cats. All will be anaesthetised except frogs.
- ROYAL COLLEGE OF SURGEONS IN IRELAND.—Three or four frogs, rabbits or minnows, the rabbit insensible the others curarised.
- TRINITY COLLEGE, DUBLIN.—None allowed.
- CATHOLIC UNIVERSITY, DUBLIN.—About eight frogs for class demonstration under curare.
- CARMICHAEL SCHOOL OF MEDICINE, DUBLIN.—About six frogs, newts, rabbits or dogs for class demonstration anaesthetised. Cannot give any answer in case of original research, except that few animals are ever used.
- PETRE STREET, ORIGINAL SCHOOL OF MEDICINE, DUBLIN.—None. The lecturers have sometime ago come to the conclusion that they are quite unnecessary.
- DR. STEEVENS'S HOSPITAL, DUBLIN.—Two or three frogs under curare for class demonstration.
- QUEEN'S COLLEGE, BELFAST.—Few for demonstrations; occasionally a larger number for original research, consisting of rabbits, cats, dogs, mice, birds, and invertebrates. About six to ten frogs used in physiological classes.
- QUEEN'S COLLEGE, CORK.—About a dozen dogs, rabbits, birds and frogs for original research and class demonstration, chiefly to discover the action of poisons—frequently anaesthetised.
- QUEEN'S COLLEGE, GALWAY.—About twelve frogs for class demonstration.

- UNIVERSITY OF EDINBURGH.—For original research forty-four dogs, eight rabbits, twelve frogs, and two cats, six being anæsthetised, forty curarised. For teaching purposes two dogs, nine rabbits, and one hundred and fifty-five frogs, all the dogs and rabbits being anæsthetised, and 133 frogs curarised.
- SCHOOL OF MEDICINE, SURGEONS' HALL, EDINBURGH.—About twenty frogs for class demonstration without anæsthetics, the animals being deprived of sensation by decapitation.
- UNIVERSITY OF GLASGOW.—Twelve frogs, six rabbits, four dogs and cats for original research and teaching, nearly all anæsthetised.
- UNIVERSITY OF ABERDEEN.—None.
- ROYAL VETERINARY COLLEGE.—About twenty to twenty-five guinea-pigs, rabbits, sheep, calves and donkeys, for original pathological research, (inoculations chiefly). No experiments for teaching.
- VETERINARY COLLEGE, EDINBURGH.—About sixty animals, consisting of rabbits, dogs, cats, mice, pigeons, frogs, horses, cows and dogs for original research and class demonstration. Nearly all the animals under complete anæsthesia.
- NEW VETERINARY COLLEGE, EDINBURGH.—Half a dozen frogs, horses and donkeys for class demonstrations—always insensible.
- VETERINARY COLLEGE, GLASGOW.—About a hundred dogs, rabbits, frogs, etc., for demonstration and original research. All animals rendered insensible excepting frogs, which were put under curare.
- BROWN INSTITUTION.—Four donkeys, thirty dogs, three rabbits, sixty-five guinea-pigs for original research, the operations being trifling. Anæsthetics were not used. Besides these, half a dozen frogs used by Dr. Klein for teaching purposes, the nature of whose other experiments could not be given.

EVIDENCE OF WITNESSES.*

Sir THOMAS WATSON, Bart., M.D.—A little pain given to a lower animal will impart knowledge to medical men to enable them to prevent human pain much more severe and lasting—experiments are justifiable, but only under great restraint and forethought—only skilful persons should perform experiments which ought not to be repeated after a discovery has been made—these may now be made nearly, if not entirely, painless—lingering pain ought never to be caused—Dr. Harvey, Sir Charles Bell and Dr. Marshall Hall, experimented and undoubtedly discovered thereby many means by which human pain, disease, and death have been averted—drugs and poisons tried on animals are unreliable, the effects being often very different indeed

* The following précis of opinions of witnesses who tendered themselves for examination is necessarily difficult to make out and imperfect, owing partly to limited space. With apologies for inaccuracies, should any be found, it is placed before readers who object to wade through much detail as a guide to the full evidence placed before the Court of Inquiry.—See Blue Book C. 1357.

upon the the human subject and upon the animal: no faith can be placed in such experiments—complicated experiments ought to be repressed—experiments on living animals should not be allowed, and are reprehensible when made, to illustrate medical teaching—there are not many persons in this country who are qualified or ought to be allowed to perform painful experiments, and these persons ought to be licensed as a check and restraint—if he (Sir Thomas Watson) had attained to eminence in his profession it had not been by experimenting, for he had never even seen an experiment performed, although he had turned the experiments of others to use in his practice.

Sir GEORGE BURROWS, Bart., M.D.—He had performed many experiments which had caused less pain than sticking a pig, as death took place in a minute or less—experiments are absolutely necessary—he had performed and seen many which have advanced medical science—some experiments must continue to be painful ones, but nearly all may now be rendered painless, and in the majority of those that cannot, the painful part, the cutting, may be done without pain—no man without sound knowledge of anatomy and physiology ought to be allowed to experiment—there have been great abuses in the performance of experiments which ought to be restrained—cases in which painful vivisection ought to be practised are comparatively rare—experiments should not be allowed in medical schools for illustration—students should not be allowed to experiment; if they follow out Sanderson's Handbook he would take steps to prevent their doing it—Harvey's discovery could not have been foreseen in its remedial consequences—vivisection ought to be restrained by legislation; the operator should be licensed by a Secretary of State, who should be advised on the fitness of the operator, both as to his attainments and his character—there has been great exaggeration in this country as to the nature and extent of vivisection.

Sir JAMES PAGET, Bart.—He entirely approved the resolutions passed at the British Association in 1871 (*see* page 150); such approval was shared by Professor Huxley and Mr. Darwin, and was signed by Professor Owen, Sir William Gull, Sir William Jenner, the President of the College of Physicians, the President of the College of Surgeons, and several other leaders in science—the discovery of anæsthetics has materially altered the nature of vivisection, as that which was simply horrible fifty years ago is now rendered painless—many of the painless experiments are of the most important kind—those which involve pain are absolutely necessary, among which are those used for testing the effects of medicines and poisons—the repetition of painful experiments is certainly not justifiable—when a scientific fact has been established painful experiments in relation thereto ought to be prohibited—students ought not to be allowed to make painful operations—it is not justifiable to perform painful

experiments before a class; but for illustration of certain truths it is necessary for students to witness the action of functions in living animals, which have been previously narcotised and opened in the laboratory, in which case the animal should be killed immediately after the exhibition—very painful experiments are very rarely necessary—experiments have been entered on sometimes before all other methods of inquiry have been exhausted—an experiment should not be made until the operator is quite satisfied that all other means of inquiry have been tried and have failed—twenty thousand persons die annually from snake bites in India—no antidote for the poison can be discovered except by having animals bitten by snakes before applying supposed remedies—it is possible that in twenty-five years a true antidote for such poison might be discovered without experimentation, but in the meantime perhaps half-a-million of persons will be killed by snake bites who might possibly be rescued by experimentation—vivisection is not performed generally by medical men—for therapeutic purposes, its results are exaggerated—Dr. Brunton discovered by experiments that nitrate of amyl checks the agony of a terrible disease, angina pectoris, and probably averts its mortal effects—laboratories at medical schools are required for work with which animals have nothing to do—there are no arrangements in London schools for keeping living dogs there, similar to those described by an architect tendering for the construction of a laboratory at the Edinburgh University—any three or four medical men could decide whether an experiment be necessary or not, and then such experiment might be allowed—public opinion without legislation will prevent abuses, but if there be legislation then operators should be licensed as teachers of anatomy are now licensed—the Home Secretary should not grant licenses without a magisterial certificate, and private practitioners should be allowed to perform an experiment required by an emergency in bedside practice without having a license—there is an exaggerated impression abroad as to the extent of vivisection—experiments have recently decreased in certain directions and increased in others—Majendie accomplished a large and excellent work by experimentation by which many human lives have been saved but he was indifferent to the sufferings of animals—better apparatus for experiments should be provided in order that pain may be mitigated—the lower animals are extremely little sensitive, and many of the experiments performed on them cause little pain—inflammation in animals may be set up with no more pain than dropping hot sealing wax on a man's finger—there are many experiments of a painful class which ought never to be repeated, but the experimenter is the only person who can judge of the necessity of an experiment—persons of high scientific character should alone be allowed to experiment—ardent physiologists perhaps put too much trust in experiments (persons studying therapeutics and diseases do so) neglecting the importance and advantage of clinical inquiry; but there is a number of things

absolutely essential for man's existence that cannot be ascertained without vivisection—experiments on animals correct observation—for instance, transfusion of blood was discovered by experiments and tried on animals first and on man after a long course of experiments—such treatment now saves many lives—if it had been tried on man first, lives must have been sacrificed during the experiments—precautions respecting the admission of air, blood-clots, &c., could not then have been discovered—from twenty to thirty persons die from chloroform in the British islands every year—a new anæsthetic is therefore needed, which cannot be discovered without experiments—medical men are constantly in the presence of persons suffering and dying from diseases that cannot be cured, and are in duty bound to ascertain remedies by any possible means, even though these may inflict on animals the same sicknesses which doctors vainly try to cure in man.

WILLIAM SHARPEY, M.D., LL.D., F.R.S.—He had performed experiments—they are absolutely necessary—anæsthetics have permitted them to be performed without pain in most cases—Galen proved by vivisection that arteries contain blood—Harvey discovered by vivisection the motion of the blood—he describes the many difficulties he overcame, and adds, “by frequent appeals to vivisection, when I first gave my mind to vivisection,” &c.—also “At length, and by using greater and daily diligence, having frequent recourse to vivisections, employing a variety of animals for the purpose,” &c.—Harvey did not discover how blood passed into the veins—Malpighi did this by means of the microscope—the next steps were made by Hales, Poiscuille, and Ludwig, by experiments—then further discoveries were made by experiments on animals referring to the circulation, so that there have been a series of experiments and a series of discoveries—vivisection has immensely advanced knowledge of the nervous system—Sir Charles Bell and Majendie made important discoveries by experiments which could not otherwise have been made—Sir Charles Bell made mistakes which have been corrected by vivisection—the effects of noxious gases upon the blood causing death have been ascertained by experiments—physiology has been greatly promoted by vivisection—physiology puts a lamp into the hands of physicians when studying disease—the treatment of heart disease has been improved by vivisection as well as surgery of the arteries, the reparation of bone after injury and disease, and the discoveries of Harvey and Bell have been followed by important medical consequences unforeseen at the time, but nevertheless consequent on vivisection—physiological knowledge acts impalpably upon the mind of physicians, and though not a reaping machine may be called a plough—vivisection is purposeless suffering unless performed by competent men—it should not be repeated when facts have been established—all experiments may be painless

during the actual cutting—there are some experiments where a certain amount of pain must be protracted after the cutting—experiments on starving animals to death should not be repeated—there is no such thing in this country as indiscriminate vivisection—few persons engage in experiments, not more than a dozen in this country—anaesthetics are employed in physiological schools—curare is not an anaesthetic, and the public should not accept it as such—Majendie's experiments were so repulsive that he declined to witness them in his youth, although studying at the time in Paris—they were very severe and without sufficient object—scientific men in this country perform vivisection with great reserve, and always with anaesthetics when possible—vivisection is not practised by students, and animals operated on are exhibited to them in the Lecture Room only when under anaesthesia—a limited number of students are admitted in the laboratory, where almost all experiments are painless—students ought to be admitted to witness experiments in the laboratory in order to learn how to perform them as physiologists in the most humane way—vivisections are justifiable for purely scientific purposes—Dr. Richardson, at his residence in London experiments on animals—curare is used to keep the animal still, and not to deaden pain—he remembered one student, when a young man, who used to perform experiments which was an exceptional case—insensibility may be produced by pithing instead of by chemical agents, by which means animals may be kept sensitive for a long time, but without pain—different animals of the same species show different degrees of capacity for pain; but all, however low, feel some pain when undergoing severe vivisections—vivisection has been practised during every period of the Christian era—when teaching anatomy in Edinburgh a bitch with a litter of pups was brought, and remained long enough to gain the affection of the people, and consequently was not operated on but sent away—students should see animals poisoned, particularly with strychnia, to recognise its effects when administered to man—one animal thus poisoned would suffice for an audience of three hundred students—it is a severe operation—experiments are far more extensively practised on the continent than in England—series of experiments are necessary to establish a doctrine—of course other scientific inquirers follow discoverers by other series of experiments to verify and correct—going in quest of one thing they find other truths—dogs and rabbits have largely profited by anaesthetics during experimentation, and therefore anaesthesia has really been a blessing to them instead of a curse; and frogs are generally operated on after the brain is destroyed—Dr. Michael Foster's experiment on "recurrent sensibility" is not necessary for exhibition at any time although he has recommended it in the Hand-Book of the Physiological Laboratory, and he (the witness) would not perform that experiment before a class—young physiologists coming home from Leipzig no doubt would introduce German methods into

this country; but for illustration experiments involving severe suffering should be abstained from—the progress of science by the discovery of anæsthetics has prevented many painful experiments formerly necessary—every new discovery widens the prospect towards the unknown—new ideas may make many experiments needful, though not necessarily an indefinite number of painful experiments—there is an apparatus at the University College like Ludwig's for keeping up artificial respiration in animals night and day if necessary, but the animals may be rendered insensible; the apparatus is worked by an engine, the engine being used for driving lathes, cutting bones and other purposes—experiments on human beings instead of on animals might be more instructive if tried, but he (the witness) does not think physiologists have any right to experiment on human beings unless the human beings are willing—Herophilus of Alexandria was denounced by Tertullian for having made vivisections on human beings—witness is not prepared to follow that example, though unprepared to say such course is not a logical conclusion from his proposition that the scientific object is the true end of physiologists—the starving of animals is a scientific experiment; he would not have undertaken them and would condemn the repetition of them—Dr. Legg's experiments on sixteen cats at St. Bartholomew's Hospital might be unadvisable—lingering pain should be avoided—young scientific men with the zeal of science upon them would be likely to commit excesses without restraint, and such persons might be restrained by law—he would not answer for Sanderson's Handbook, as to the experiments recommended and the non-employment of anæsthetics—interference with scientific inquiry would be an act of barbarism committed by humane people in order to prevent what they conceive to be barbarity—experiments on living animals have not the effect of blunting the feelings and hardening the natures of those engaged in them—experimenters perhaps ought to keep a record of painful experiments for the supervision of the authority who may license them, but he would prefer to trust to the influence of public opinion and the example of leading physiologists to prevent excesses—Haller stated that “a single experiment will sometimes refute the laborious speculation of years”—Dr. Stark, Dr. Parkes, Sir James Simpson and other medical men have made experiments on themselves.

Professor GEORGE M. HUMPHRY.—As a large part of the animal kingdom maintains its perfection by inflicting pain and death on other animals it is justifiable for man to inflict pain and death on other animals when he may reasonably hope to benefit his condition thereby; therefore vivisection is justifiable, but it should be always painless when possible—nearly all experiments may be made painless, and painful ones need not be performed often—only competent persons should be allowed to operate—many pathological experiments

cannot be made painless—by such experiments tubercle diseases are becoming better understood, and hopes are entertained that cancer will eventually be prevented by discoveries—vivisection is performed only by competent men in this country—he gave up shooting for pleasure because he caused lingering suffering on animals—students ought not to be taught to practice vivisection alone—experiments are not going on improperly in this country as represented—the experiment quoted from *The Doctor*, June 1, 1874 (see page 140), is meaningless (see *Handbook for the Physiological Laboratory*, page 160)—anæsthesia cannot be applied to pathological experiments where the processes have to be observed during days or weeks—if all knowledge gained by experiments on animals were blotted out medical science would be in a state of comparative barbarism—in the laboratory at Cambridge students are never permitted to experiment except when the animals have been narcotised and then only seniors and for some definite purpose—lower animals do not suffer so much pain as man—when dogs are fastened down and are unable to move they evince very little suffering—curare is not an anæsthetic ; it arrests the functions of the motor nerves but not the sensory nerves—the experiment at page 403 in the Handbook alluded to will rarely be necessary to be performed or exhibited—there certainly are not more than half-a-dozen schools of practical physiology in this country, and as editor of a journal of anatomy and physiology he knows all who are at work at vivisection—there are only one or two persons carrying on pathological experiments—important results of experiments are made known in medical journals—experiments are justifiable for the purpose of extending science as well as curing disease because science has relation to the advancement of man.

Professor PRITCHARD.—No operations are allowed at the Royal Veterinary College as experiments—such experiments would be very improper—veterinary surgeons do not perform them in this country—the dog's circulation being intermittent, chloroform cannot be used with even so much safety as on human beings—students at the College are not allowed to operate—pathological experiments have been tried at the college for glanders in horses, and typhoid fever in pigs, and fluke in sheep—thousands of ordinary surgical operations have been performed by himself in the case of animals operated on and he has never detected any difference in sensation between the higher animals and man—frogs would probably be equally sensitive, because the irritation of the skin by a parasite is the same in smaller as in larger animals—experiments have been tried to prevent the ravages of the trichinæ in pigs, but he believes without success—is opposed to the introduction of a physiological laboratory at the college—there are plenty of diseased horses, and healthy ones need not therefore be made diseased for the purpose of experimentation, and such animals are more suitable for experiments in the healing art than those inoculated

with disease—experiments as to cattle plague and pluro-pneumonia have failed to produce good results, and those tried by administering drugs to animals cannot be of much value to man, as the effect produced on animals is different in man and animals.

Professor H. WENTWORTH ACLAND, M.D., F.R.S.—Human knowledge has been greatly advanced by experiments, e.g., the circulation in living animals in reference to which human speculations were transformed into absolute certainty—severe suffering ought not to be given except to establish new and important doctrine—most, nearly all, experiments may be performed now without pain—needless experiments should not be performed whether they give pain or not—he would not put to death the meanest animal needlessly, and would object and consider it an immoral act to destroy an animal to show a thing which can as well be shown by a diagram—abroad animals are often operated on with unscientific carelessness, which would not be endured in England—biologists perform experiments without being medical men, and their number is greatly increasing—a profession has thus been created devoted to pure research without reference to curing diseases—he does not believe that English scientists are guilty of careless and wanton cruelty—he knew of no students guilty of wanton experiments—only the most able men should be allowed to vivisect, except students under training as future physiologists—the progress of medicine does not depend mainly upon experiments on animals, though it is assisted by them—has never performed an experiment causing pain without great necessity—the “Handbook of the Physiological Laboratory” was published, he believes, to teach learners “how they may themselves pursue experimental investigation”—the Royal Society presented the Copley medal to Sir Benjamin Brodie for experiments on living animals, and its transactions are full of experiments during the last two centuries—seeing the action of structures of living beings gives quite a different conception to that obtained from the dead body—vivisection is growing into an extensive practice in this country—the moral punishment which a reckless experimenter would receive in this country may make legislation unnecessary.

Sir WILLIAM FERGUSON, Bart., F.R.S.—Vivisection has been of service to human knowledge, but not so much to human nature—resort to it is increasing—it should not be allowed except to decide some question of doubt and difficulty, and then it requires a high style of judgment—few persons are qualified to experiment—the most striking experiments on animals referring to surgery were previously performed on the human subject and were therefore unnecessary; e.g., John Hunter’s most brilliant performance was done on the human subject by him, and years afterwards was copied on animals—no great vivisector has made himself a great surgeon by vivisection—Mr. Syme lived to

express an abhorrence of vivisection—he (witness) would not perform experiments now which he did when young—experiments have not tended much to mitigate human pain—chloroform was discovered without experimentation on animals—in some cases vivisection is performed in a manner calculated to lower the reputation of scientific men—legislation might do good, but supervision ought to be insisted on in order that reckless men may be punished by public indignation and stigma—knowledge has resulted by experiments on the blood, heart, liver, kidneys, and nerves ; but some of it is of no great value—the abnormal conditions produced by experiments render deductions of doubtful advantage—he would not totally abolish vivisection, but there should be superintendence,—abroad the hospitals are private and surgeons are not under surveillance—English hospitals are under surveillance—the practice of the English system tends to greater humanity in the treatment of patients—curare is a poison and cannot be called an anæsthetic—experiments under anæsthesia do not yield good results, because the animal is not in a normal condition, and if not killed during anæsthesia it suffers great pain when sensibility is restored during the remainder of the experiment—experiments were legitimately performed by eminent men such as Bell, Cooper, Brodie, Travers, Lawrence, and others—Jones's experiments were of considerable advantage to surgery at the time—witness's own knowledge had been derived by clinical and pathological observation on human beings—the grounds for believing that vivisection is being carried on improperly are statements of pupils as to what they have seen at certain laboratories—he cannot say where those laboratories are situate, or prove anything ; he only states impressions—he does not think there is much amateur experimentation going on—zealous talented men should not be restrained in experiments if they do not commit excesses, but they should be compelled to register their experiments and admit inspectors into their laboratories.

ALFRED SWAINE TAYLOR, M.D., F.R.S.—He had performed many experiments on animals for evidence in criminal cases, several of which had benefited mankind—there is much unnecessary experimentation in great trials—strychnine acts in a similar manner on all animals furnished with spinal marrow—so many people die from strychnia that experiments are not now needed—pain in poison experiments need not be protracted—he read the experiments made at Norwich which led to the prosecution instituted by the R.S.P.C.A. and had no doubt they were of a most cruel kind, and unjustifiable—anaesthetics cannot be given in toxicological experiments—the animal most like a human being under the effects of poison is the dog and therefore experiments on that animal yield the best toxicological results—in contradiction to the opinion of Sir James Paget he does not believe that antidotes for snake bites can be discovered by

experiments, although he had considered the matter with great care—some experiments involve only the drawing of a drop of blood—it is not necessary for students to see the action of strychnine on animals to enable them to make a correct diagnosis in poisoned human patients (strychnine causes agonising pain, very dreadful to witness)—coloured wax models and drawings are sufficient—any man who has received a proper medical education can detect the presence of strychnia without experimenting—students are not allowed to perform experiments at Guy's Hospital—curare is a very doubtful anæsthetic—he does not perform more than twelve or fourteen experiments per annum—the French are much more reckless in vivisection than the English—he studied in Paris—the experiment at p. 108 of the Handbook is a very cruel one, without any good purpose.

GEORGE ROLLESTON, M.D.—Experiments involving pain should never be resorted to except when necessary, and then only in the presence of very few persons, and anæsthesia should invariably be employed when possible—he made a report to the British Association on this subject and drafted the resolutions (see p. 150) passed in 1871—to those resolutions he would now like to add that life should be extinguished after vivisection to prevent the animal awakening up into pain—experiments are necessary for medical jurisprudence, testing poisons and drugs, and a few surgical operations—human functions have received considerable elucidation from experiments and light has been thrown by them on the most terrible diseases—students should not be allowed to experiment, except those about to devote themselves to physiology—experiments under anæsthesia may be exhibited to students only on a few cardinal points—vivisection is likely to lead to carelessness, the impressions of suffering growing weaker by habit as the desire for discovery grows stronger—Haller in his later age reproached himself bitterly for his vivisections which caused him great anguish of conscience, and a living *savant* of great eminence had expressed himself in a similar manner to the witness—legislation is expedient to diminish the number of experiments, to insure the employment of anæsthesia in almost every case, to prohibit painful demonstrations—returns of experiments should be made—there would be no inconvenience in compelling all experiments to take place in a properly inspected laboratory—the experiments at p. 403 and 490 of the *Handbook for the Physiological Laboratory* are entirely needless to repeat—the Handbook obviously encourages the performance of such experiments—curare is not a safe anæsthetic.

JOHN SIMON, F.R.S.—A Government annual grant of two thousand pounds is used in pathological experiments on animals principally—bowel complaint has been given to mice to discover the cause of cholera—in the performance of experiments directed to be made in the Privy Council Department conscientious regard has been

had to the mitigation of suffering—experiments ought to be confined to competent persons—severe operations are fewer now than formerly—anæsthesia has yielded this as nearly all of these may now be performed without pain—the public have been misled by exaggerated statements on this subject—any law to restrict the physiologist in relation to experiments on dogs should restrict him in experiments on other animals—to prevent cruelty the present statute might be extended to wild animals—Dr. Snow in 1849 taught a theory for preventing the spread of cholera which has been demonstrated to be true by the Privy Council experiments, namely, that choleraic discharges will produce cholera—sheep-pox experiments have been extensively tried, but with no hope of cure at present—the same may be said with regard to giving consumption to healthy animals—experimentation on animals saves man much danger—surgery has progressed immeasurably by vivisections especially as regards inflammation, fever and ligatures of arteries—students are not allowed to experiment at St. Thomas' Hospital—certain experiments should be shown in the class room, but only painless ones—society at large uses animal life to an enormous extent—why should science be forbidden painless experiments?—surgeons who do not experiment on animals experiment on their own patients—important facts relating to pyæmia have been discovered by experimentation—experiments relating to biliary fistula are sometimes open to fallacy, but experimenters guard against fallacy as much as possible—limiting experiments to special places, special persons and inspection would induce physiologists to take refuge in other countries—it is monstrous to legislate against physiologists and to leave sportsmen to inflict much worse sufferings for sport—frogs have full susceptibility of pain—physiologists would not recklessly hurt even frogs—they are nevertheless tortured.

JOHN COLAM, for R.S.P.C.A. (see full minutes of examination and documentary evidence, p. xvii.-xlviii. and 1-150).

ARTHUR DE NOE WALKER, M.D.—He had been in communication with the R.S.P.C.A., who requested him to appear before the Commission and give evidence—his impression of experiments in foreign laboratories is that much “wanton and unnecessary” cruelty is practised—in this country some experiments, though not of this character, are still to be condemned and these are greatly extending—the practice requires legislative restraints, which may be done without interference with research—if experiments have led physiologists to the discovery of the circulation of the blood did they ever suggest to any of them that digitalis, aconite, spigelia, belladonna, iodide of potassium, &c., would affect the heart and circulation; but physicians having informed physiologists of these facts the latter now tell us why and how they act—physiology, *i.e.*, a knowledge of healthy functions has never discovered one remedy for disease, although it has

sacrificed millions of animals under intense suffering—did it suggest that lemon juice would cure scurvy? &c., &c.. [many others named]—Professor Rivière, of Montpellier, said with truth “the discovery of the circulation of the blood has not advanced medicine a single step” (he ought to have said therapeutics); at most it can only point the physician to the seat of disease—transfusion of blood has saved many lives, but that invention is not due to vivisection—no experiments should be allowed without license except in certain forensic cases—the number of animals allowed to a vivisector should be limited—the operator should be compelled to send in two different returns, the first shewing objects, which will be condemned if not justifiable and the second results obtained; also shewing whether anæsthetics have been used or not—frogs are kept in closed jars for months until ulcers form round the mouth and nose—they excite no pity—vivisections for demonstrations should be abolished—curare is not an anæsthetic—he would entirely abolish vivisection if possible, but as that is not possible he would put it under legal restrictions—since 1850 experiments may have discovered the cause of the second sound of the heart, and that result is very useful—the functions of the nervous system have been discovered by experiments, including the sensory nerves and the spinal cord—all our knowledge of first processes is due to experiments, but such knowledge has not revealed remedial agents—pathology has—discovery of the cause of disease may lead to cure—ringworm was formerly improperly treated—now that the cause has been ascertained to be a parasite, chemistry has suggested a rational treatment—the eye instructs the mind, but experiments are not needed to teach students by the eye as physiology may be learned without them—he is not a competent judge of vivisection in this country—if suggested experiments were previously submitted to a scientific board useless experimentation would be prevented—nature is not philosophically interrogated but violated by many of these experiments, and deductions are therefore unsound—the effect produced on himself when a continental physiological student, prevented enjoyment of cigar and dinner—he could not get rid of the imploring look of starved dogs who cried for food when they saw human beings, the patient suffering of fowls, the desperate efforts of rabbits to allay the pangs of hunger—at this time he used to dine with a lecturer on physiology abroad and could not eat his dinner, but the physiologist could; the physiologist had been a vivisector some years, and only smiled and shrugged his shoulders when he, the beginner, expressed his compassionate feelings—professional vivisectors ignore every appeal for pity—they see no kind of abuse and very often no pain—they, like all mankind, are not exempt from the law of habit.

LAWSON CAPE, M.D.—Had been in communication with Mr. Jesse, but is not a member of his society—vivisection of highly organised animals should be abolished—licensing of vivisectors ought

to be guarded by stringent regulations—certain experimenters might be licensed—had not seen vivisection in England but had abroad, where they were very cruel.

GEORGE MACILWAIN, F.R.C.S.—During his long practice he had studied vivisection—believes there is nothing useful in that practice—it is not a fact that Harvey discovered the circulation of the Blood by experiments on animals—his experiments were only to demonstrate the conclusions of inductive reasoning—Hunter performed many experiments, but his conclusions might have been more clearly proved in ordinary practice—his discovery as they call it for the relief of aneurism was not arrived at by experiments—animals do not have aneurisms, and although he tried he could not make one in an animal—experiments do not conduce to the cure of disease—he proved this in a book written by himself—Mr. Travers made experiments on dogs in reference to strangulated hernia—his erroneous conclusions therefrom were mischievous—witness pointed to the error many years ago, and proved that it caused perhaps the most painful results, and thenceforth Mr. Travers' practice was changed—vivisection is a fallacy in medical investigation and ought to be abolished.

The Rev. SAMUEL HAUGHTON, M.D.—In consequence of having signed the memorial against vivisection presented to the R.S.P.C.A. Mr. Colam wrote him a letter (quoted in full) asking for the favour of data, upon which the allegations of the Memorial signed by himself were founded—he declined to be a detective, but replied to the effect that experiments had conferred invaluable benefits on medicine and physiology; that scientific men would not come forward to give evidence against their brethren, who are not sensitive when inflicting pain on animals or men, although very sensitive to pain affecting their own persons; that the University of Dublin had prohibited vivisection; and that the true use of experiments is for original research, and should be employed with care and conscience—vivisection for research cannot be prohibited, but should be supervised by legislative enactment; because (1) he would not trust the conscience and common sense of physiologists; (2) a large proportion of experiments now performed upon animals in this country are unnecessary repetitions of German experiments; (3) many experiments which ought to be made are cruelly performed, *e.g.*, Dr. Magnan's experiment at Norwich, which was undoubtedly performed with unnecessary cruelty; (4) experiments are undertaken without due deliberation and previous forethought; (5) investigators exaggerate their own hobbies, and particularly the importance of results obtained by experiments—Dr. Hughes Bennett made most extensive experiments on dogs whose livers were subjected to the action of mercury—his conclusions may be useful in treating sick dogs, but

they do not apply to men at all—during the trial of Palmer he experimented with strychnia and nicotine, and made discoveries relating to frogs, which produced no results at all when applied to dogs—but the experiments yielded a good result by pointing to a remedy for strychnia, namely, tobacco (nicotine)—Lord Henniker's bill was defective in its definition of vivisection, and would therefore allow many experiments—anaesthesia is often applied imperfectly and dropped altogether when the controlling eye is absent—public feeling should be respected as much as the feelings of animals—reliable inspectors should be appointed either by the R.S.P.C.A., or by the authorities—the dissecting room degrades some students and elevates others—his long experience proves this, and he would therefore shrink with horror from introducing them into laboratories to witness painful experiments which would demoralise them—science would gain nothing, and the world would have let loose a set of young devils—licensing vivisection, and placing them under proper supervision, and forbidding experiments except under such supervision, would mitigate cruelty, but extending the Anti-cruelty Act to all animals for the purpose of preventing vivisection would be ineffectual, because evidence could not be got without inspection—without inspection you cannot enforce anaesthesia, with it you can—physiologists should not be inspectors, for the public ought to be represented as well as science—he felt very strongly that control should partly be under public opinion.

ALFRED HENRY GARROD—He experiments on animals for scientific investigation, but in no case does he cause pain—he had not seen an incompetent person perform experiments—vivisection is not necessary for educational purposes—students if industrious can learn all that is required without witnessing experiments—he has had much experience in applying anaesthetics—there is no real difficulty in producing absolute insensibility when the operator is competent—he had perfectly anaesthetised the largest ruminant, namely, the giraffe—when under pain animals always indicate it more or less vigorously; when under imperfect anaesthesia they manifest it feebly; when under perfect anaesthesia there is no sign of pain—a humane operator can therefore always prevent pain in operations where pain is not an essential object—he emphatically denies that anaesthesia is a delusion or in any way ineffectual.

FREDERICK W. PAVY, M.D., F.R.S.—Vivisections may be made almost entirely without pain—during twenty years' lectures at Guy's Hospital he had illustrated to students on living animals previously made perfectly insensible—experiments are much more numerous on the Continent and of a nature which would not be tolerated by public opinion in this country—he has never known experiments to be made by medical students, and does not believe they experiment—if that could be proved they should be stopped—anaesthesia has been

they do not apply to men at all—during the trial of Palmer he experimented with strychnia and nicotine, and made discoveries relating to frogs, which produced no results at all when applied to dogs—but the experiments yielded a good result by pointing to a remedy for strychnia, namely, tobacco (nicotine)—Lord Henniker's bill was defective in its definition of vivisection, and would therefore allow many experiments—anaesthesia is often applied imperfectly and dropped altogether when the controlling eye is absent—public feeling should be respected as much as the feelings of animals—reliable inspectors should be appointed either by the R.S.P.C.A., or by the authorities—the dissecting room degrades some students and elevates others—his long experience proves this, and he would therefore shrink with horror from introducing them into laboratories to witness painful experiments which would demoralise them—science would gain nothing, and the world would have let loose a set of young devils—licensing vivisection, and placing them under proper supervision, and forbidding experiments except under such supervision, would mitigate cruelty, but extending the Anti-cruelty Act to all animals for the purpose of preventing vivisection would be ineffectual, because evidence could not be got without inspection—without inspection you cannot enforce anaesthesia, with it you can—physiologists should not be inspectors, for the public ought to be represented as well as science—he felt very strongly that control should partly be under public opinion.

ALFRED HENRY GARROD—He experiments on animals for scientific investigation, but in no case does he cause pain—he had not seen an incompetent person perform experiments—vivisection is not necessary for educational purposes—students if industrious can learn all that is required without witnessing experiments—he has had much experience in applying anaesthetics—there is no real difficulty in producing absolute insensibility when the operator is competent—he had perfectly anaesthetised the largest ruminant, namely, the giraffe—when under pain animals always indicate it more or less vigorously; when under imperfect anaesthesia they manifest it feebly; when under perfect anaesthesia there is no sign of pain—a humane operator can therefore always prevent pain in operations where pain is not an essential object—he emphatically denies that anaesthesia is a delusion or in any way ineffectual.

FREDERICK W. PAVY, M.D., F.R.S.—Vivisections may be made almost entirely without pain—during twenty years' lectures at Guy's Hospital he had illustrated to students on living animals previously made perfectly insensible—experiments are much more numerous on the Continent and of a nature which would not be tolerated by public opinion in this country—he has never known experiments to be made by medical students, and does not believe they experiment—if that could be proved they should be stopped—anaesthesia has been

quite complete when used by himself—if it were not complete the operator, apart from the question of humanity, would make it so to facilitate the operation—chloroform is repeated sometimes for two or three hours—animals are bought in Leadenhall Market for experiments—painless experiments as illustrations are absolutely necessary—he introduced them into his lectures after returning from his studies in France—wanton cruelty in this country by experimenters is unknown—the time of scientific men is too valuable to be wasted wantonly—he had been much assisted in discovery by experiments—it is necessary for students to see healthy functions as well as diseased functions—painless physiological experiments supply the former and the visitation of hospital wards the latter—disease must be seen and not merely read of in books, and health likewise—there is no essential difference in fundamental points between men and animals—anæsthetics may be used even with strychnine—some of the experiments of the Handbook alluded to would not be allowed at Guy's Hospital—the experiments in such book are chiefly derived from continental sources and should not be made by students—there is not the slightest difficulty in the application of anæsthetics to produce absolute insensibility. (Further opinions of Dr. Pavy will be found at p. xxv.)

PHILLIP HENRY PYE-SMITH, B.A., M.D.—He almost entirely agreed in the opinions of Dr. Pavy—abuses exist abroad which, if imported, would make legislation necessary—in England complete anæsthesia is produced except in a very few experiments, and invariably by himself in all experiments—undoubtedly it can be made complete, and when not complete the operator is incompetent—he had never seen an experiment performed without anæsthetics.

J. BURDON-SANDERSON, M.D., F.R.S.—Physiology has progressed considerably lately, and schools have been organised for teaching it—until recently it had been much neglected in this country—many experiments performed on the Continent ought not to be done—there sentiment is different, and consequently practice—generally Dr. Playfair's Bill was satisfactory—anæsthetics ought always to be used when possible—science requires the repetition of experiments—only the most scientific persons should be allowed to experiment—animals exhibited to students are anæsthetised before and during experiment in every school of the kingdom, except in a few toxicological and pharmacological instances—his experiments give very brief pain to animals—the *Handbook for the Physiological Laboratory*, edited by himself, was intended for physiological and not for medical students—regrets greater care had not been given in writing certain phrases therein, as much misconception has arisen in consequence—to the words “intended for beginners” should have been added “in research”—his object, and those acting with him, was to discourage amateur experiments and to promote a school of physiology, the

students in which would be properly trained to operate with skill and without cruelty—curare is not a safe anæsthetic, but when administered to frogs, the animals are almost insensible to pain—experiment No. 4, p. 269, in his book, is imperfectly described—it undoubtedly should be performed only after the destruction of the brain—the expression “slightly” curarised is an oversight—the experiment on p. 298 was a painful one—anæsthetics should have been enjoined in a more general way at the beginning of the book, but the persons for whom the book was intended would understand without direction that experiments must be performed under anæsthetics whenever possible—in future directions to beginners great care shall be taken—curare is not used as an anæsthetic, but it is frequently used with an anæsthetic, the former to destroy the action of motor nerves, and the latter of sensory nerves—experiment No. 101, p. 308, is painful, and ought not to be performed except for research—many things in the book are not intended for repetition—the book will not add to the number of inquiries and experiments—experiments on asphyxia made by the Medical and Chirurgical Society some years ago, (see pp. xviii- and 49, No. 77) led to very unsatisfactory results—in London practical physiology is taught only at Guy’s Hospital, King’s College, and his school—the only necessity for causing pain is for original research—experiments have yielded much knowledge recently with respect to inflammation, tuberculosis—physiology is destined to be the foundation of medical science and the guide of practitioners—the application of cold as a remedy against fever has been revived entirely by experiments, and lives have thereby been saved—experiments have also shown the absurdity of venesection—they have thrown much light on the value of certain kinds of food—also respecting the value of alcohol in fevers—experiments have taught us that tuberculosis proceeds from constitutional predisposition as well as exciting causes, and will ultimately lead to the prevention of these diseases—his appointment at University College compels his devotion to original research, and precludes him from private practice—schools of physiology ought to be attached to every scientific school, the students in which should work under a professor—such schools are managed by a committee of gentlemen, and are amenable to public opinion in reference both to science and to humanity—there is nothing objectionable in a licence—inspection is not possible—a register of animals cannot be kept with good effect—with Dr. Pavy and Dr. Pye-Smith he would say that everything performed by physiologists is without secrecy of any sort so far as the Royal Society for the Prevention of Cruelty to Animals are concerned, who might come and see the experiments—experiments are announced on a black board, so that anyone may attend—has no objection to such publicity—that applies only to his fixed lessons, and not to the laboratory work—the public might know even what goes on in the laboratory, but vivisectors would not like to be intruded on at work

—the present feeling in the public mind, he is sorry to find, had arisen in great measure from his book, and in order to correct this he should be glad to insert in all future copies to be sold the recommendation of the Committee of the British Association relating to vivisection (see p. 150) and he should not object to the Commission making strong expressions in accordance with his own regrets respecting the unfortunate omissions in the Handbook ; indeed, he should be glad for such regrets to be made public.

MICHAEL FOSTER, M.D., F.R.S.—He agrees with Dr. Burdon-Sanderson, except that he does not see any necessity for legislation—when he wrote his portion of the Handbook alluded to he appended a preface, in which he stated that the experiments were such as the student might perform under “due supervision”—he was not particular to insist on the use of anæsthetics, and therefore, in some cases, directed them to be used, and in other cases did not enjoin their application—if he had to write the book again he certainly would be more careful—in his own experiments on frogs he uses morphia with curare to prevent pain—he gives demonstrations to his pupils only in those cases where anæsthesia can be applied, and such course ought to be followed, and he believes it is—legislation would prevent private experimentation, which he thinks a disadvantage—he knows only one person, Mr. George Henry Lewes, unconnected with a laboratory who is experimenting—he feels a great difficulty as to licensing ; but it must be a general license if at all.

J. BURDON-SANDERSON, M.D., F.R.S.—(Further examination.) The following gentlemen are engaged in performing operations on animals :—Dr. Yule, Dr. McKendrick, Professor Dewar, Professor Rutherford, Professor Arthur Gamgee, Dr. Caton, Dr. Michael Foster, Dr. Pavy, Dr. Pye-Smith, Dr. Brunton, Dr. Ferrier, Dr. Fraser, Dr. Crichton-Browne, Dr. Klein, Mr. Garrod, Dr. Wickham Legg, and Dr. Burdon-Sanderson—if there are other investigators he would know their names—students do not make experiments which involve too much exactitude and preparation to tempt them to give up their leisure—a student of Dr. Klein’s has a laboratory, and another private gentleman has made several experiments of a useless kind—witness has management of the Brown Institution—experiments made there relate to histology, but certain experiments have been made there on inflammation before Dr. Klein’s private pupils—it is not possible to say that physiological experiments have not been made there, but those which have been made have no connection with the business of the Institution—the object of the Institution is to investigate and cure the diseases of animals—extensive investigations have been made there, particularly with reference to tuberculosis, nature of fever, cancer, and typhoid fever—many experiments have been carried on there with reference to sheep pox, the object of which is to

acquire a knowledge about small pox—the results will not diminish sheep pox—the races of dogs and sheep will not gain anything by the sufferings which dogs and sheep have undergone in experiments at the Brown Institution, except indirectly, for knowledge is beneficial to the dog as well as to man—many of the experiments in the Handbook edited by himself refer to results obtained, and not to demonstrations which should follow—their recital prevents the repetition of experiments by students and others—the Handbook is an objective basis of physiological doctrine—physiology has been advancing in proportion with the advance of physical science—the tendency of physiological experimentation is to introduce expensive instruments, out of the reach of private persons, and requiring special buildings for their use—there are very few experiments in the Handbook which can be conducted without exceptional instrumental aid—the distinct tendency of laboratory instruction is to prevent useless experimentation—the object of Dr. Playfair's Bill was to prevent cruelty during experimentation—it did not attempt to restrain cruelty going under the name of sport—pain caused during scientific investigations is inflicted on animals with reluctance, for the accomplishment of high purposes; pain caused for sport is inflicted without reluctance, and for pleasure only, with no high purpose—he would justify the infliction of pain for the advance of knowledge, but on moral grounds he would oppose it in the absence of such definite object—if a man made a hare run round a covered gallery, and set off a machine to frighten the hare very much by running after it until the animal had gone through agonies of fear and exhaustion, and then proceeded to crush its life out, there ought to be some important truth to be gained thereby before the experiment would be justifiable—if he did this for pleasure every physiologist would visit him with reprobation—yet that experiment would be coursing—when rabbits are snared or trapped by countless thousands they are seldom killed quickly, but remain in agonies for a long time—their cries are heard night after night—torture inflicted by the gunshot wounds of sportsmen is much greater than anything inflicted by physiologists—he had examined hares in whose serous cavities shot corns had lodged, producing very painful protracted inflammation—it would be impossible to estimate the amount of suffering so inflicted—students are sometimes men practising as physicians or surgeons—medicine stands at present on experience, and the most competent man, therefore, is the man possessing most clinical knowledge—he had used from 200 to 300 rabbits in his experiments relating to tuberculosis only—the number used in other researches must have been extremely small, except in one, which consumed fifty animals—he does not think it reasonable to fear that if experimental schools increase in this country continental practices will be imported—it would be improper to repeat Paul Bert's experiment of dissecting out and irritating the pneumogastric and sciatic nerves of a dog submitted to artificial respiration, the

engine working on the dog all night—also Chossat's starvation experiments, but he would not exclude further verification in these if necessary—the experiments of baking animals to death might be made useful and not very painful—when an animal is subjected to high temperature pain ceases—the same thing applies to freezing animals to death—these should not be repeated when the results are established—during the last twenty-five years physiologists have been engaged in great measure in revising experiments previously made—old experiments suggest new ones not likely to be less painful than old ones—Dr. Ferrier performed experiments at the Brown Institution, which have been discontinued—Dr. Klein's experiments are conducted at the Brown Institution in a laboratory—he can not say how many patients are kept at the Brown Institution—guinea-pigs, rabbits, and rats are kept there for experiment—there are more animals kept there for painful and painless experiments than for cure as patients—he takes guinea-pigs from the store of the Brown Institution for experiment elsewhere if he requires them—Dr. Klein draws from the store also for his experiments at the Institution—the animals are kept for any purpose for which they are wanted—at University College dogs and cats are purchased by a servant, by what methods he does not know.

JOHN ANTHONY, M.D.—He was selected from seventy pupils, by Sir Charles Bell, to perform his dissections—he never saw Sir Charles operate on living animals, but he attended his demonstrations which were on the dead subject—he studied in Paris—the English students were then indignant at the cruelties of vivisection they saw there—experiments are absolutely necessary; and efficient experimenters should be licensed—perfect inspection should be enforced—public demonstrations should be prohibited, as they have no good end, but demoralise spectators—a public laboratory would not answer—even painless class demonstrations on living animals ought not to be allowed—all valuable work is done in the quiet of the laboratory—he never gained one single fact from seeing those cruel experiments in Paris—instead of *ateliers*, or galleries [as proposed by Dr. Hoggan], to which the public should be admitted, which would encourage morbid curiosity of a certain class of persons who try now to see operations at hospitals, each experimenter should have his own room, and the general public should be excluded, but he would give inspectors perfect access at all times to those rooms—the greater number of experiments may be entirely painless—those where pain would formerly have been inevitable throughout may be painless now during the severe part of the operation—experimenters are on the threshold only of their profession—much increase of experiment may be expected—animals beneath mammals do not suffer so much pain as is supposed—for experiment animals of a low range should be taken—it is God's merciful arrangement that these animals

which are the prey of others are not capable of great pain—dogs and cats are used because they are cheapest of the higher mammals, and easier got—but many experiments would be useless if not on animals of most perfect nervous organisation—it is not necessary for students to see experiments with strychnine—they can surely learn when a medicine is a purgative without proving it on animals—the effects of strychnine on the human body are of the coarsest phenomena, and in practice could not possibly be taken for anything else—fond as he is of physiology there are certain experiments published which he would not do for the world—experiments have sometimes been prompted by curiosity—he knows young medical practitioners who have done these—he has no actual knowledge of them, but has only heard them spoken of—the most painful of these is one on the generative parts, the fallopian tube of guinea pigs, in which the pain is caused by a small slit in the skin; much less than is caused by “spaying” sows to fatten them faster—does not know of any worse experiment by students, and this only on hearsay—animals suffer less than humans because they are not subject to the hyper-æsthesia of civilisation, to anticipation, and to constitutional irritation—vivisection hardens operators as to the sufferings they cause—Sir Charles Bell was very proud of his experiments—it is an extraordinary statement that he set no value on them—on the contrary, he quoted them as the only means by which he had arrived at his conclusions—hyper-æsthesia exists in higher breeds of lower animals as well as in man—if dogs and horses were excluded from experiments, all the highest suffering would not be got rid of, because hyper-æsthesia exists in other species—it is intelligence which governs feeling, and those animals nearest to man in intelligence are the best subjects for experiments in matters relating to man—some animals have power to anticipate pain—a dog when once whipped does so.

WILLIAM RUTHERFORD, M.D.—At the Edinburgh University lectures, animals are used under anæsthesia (except to a few frogs) produced by opium, chloral, and chloroform—frogs are generally stunned on the head and pithed—in the practical physiology class rabbits and frogs are used, the rabbits being entirely insensible—for original research he, or his assistant, or some competent physiologist performs experiments, one-half of which perhaps are made insensible to pain—anæsthesia is always used except when it vitiates the result—repetitions are justifiable when previous experiments have to be tested—there has been a considerable increase lately of vivisection—public feeling has been misled by attributing foreign experiments to English experimenters—foreign people are not moved by the high tone prevailing on this subject in England—as a student, and throughout his life, he had never heard of students performing experiments in private rooms secretly—painful experiments are justifiable for original research and verification—it is not necessary to show

pain to students—only thoroughly competent persons should be allowed to perform painful experiments—he was educated as a physiologist at Berlin—the same experiments would teach a hundred students as well as one; thus humanity suggests an organised system of teaching physiology—animals, and even men, are not equally sensitive—sheep-dogs are not so sensitive as other dogs—cats are, very—he never experiments on spaniels, greyhounds or cats for that reason—guinea-pigs are tolerably sensitive—Dr. Playfair's bill was much more objectionable than Lord Henniker's, as it prevented, while the latter allowed, demonstrations to students—an experiment performed by him (see *Medical Journal*, Oct. 23, 1875) was done under curare—it proves that rhubarb and colchicum stimulate the liver—those medicines were believed to have these qualities, which now is proved—the cutting operation lasted half-an-hour, and the observation, during which there was much pain, lasted eight hours in each case, and the previous fasting lasted eighteen hours—it does not follow that the result obtained on the dog would be the same on man—curare stops the dog from crying—legislation might reasonably include provisions for registers of experiments being kept—he studied experiments at Berlin eleven years ago, which he could not then have seen in England; but now he can do so—it is a matter of opinion whether, by introducing foreign experiments, the tone of physiologists in England may be lowered to that described as existing abroad—it is a matter of opinion in which all are not agreed as to the number of necessary experiments of a painful kind—at Berlin and Leipsic painful experiments were shown which he would not show—during 1874 he used forty dogs and eight rabbits for original research—it would be a great blow to the teaching of medicine if dogs and cats, and horses were excluded from experiment—notwithstanding his painful experiments performed to test the action of calomel, opinion is still divided, and many more experiments may be needed—curare is not an anæsthetic, but it deadens pain to some degree.

WILLIAM TURNER, M.B.—To vivisection mankind owes much in medical science; circulation of the blood was discovered, as well as the lacteal and lymphatic system of vessels, and the compound function of the spinal nerves—(long and interesting extracts were read relating to Harvey, Malpighi, Asellius, Pecquet, Walker, Sir Charles Bell, Dr. Jones, Sir William Lawrence, Professor Spence, Hunter, Jenner, Tanner and Galvani)—the whole history of vaccination is based on experiments on animals; and galvanism, as used by medical men, and in telegraphy, electro-plating, and light-houses is founded on experiments on animals by Galvani—the majority of experiments may be painless—experiments before students should be limited and painless—competent persons alone should experiment, and there are few in this country who are competent—others besides physiologists are engaged in vivisection who would be restrained by legislation, and this

would lead to difficulties and complications in industrial occupations, particularly on the farm—a border farmer recently told him that he castrates about 300 lambs every year, and cuts a portion off their tails, and snips their ears—carried on over the whole country this kind of vivisection is enormous, and similar vivisections take place on cattle and horses—he does not believe that any of his 468 pupils experiment—generally there is a repugnance to vivisection—as to digestion, secretion, distribution of the blood, nervous system, action in the kidneys, reproductive functions, we should know little except for experiments—Germany yields more discoveries than any other country—experiments have conduced much to information on asphyxia, reproduction of bone and diabetes—an essential condition of experiment is that the animal be subjected to as little pain as possible.

JAMES T. CRICHTON BROWNE, M.D.—He recently performed certain experiments on forty-six animals and discovered a remedy thereby—Dr. Ferrier performed twenty nine experiments at his laboratory—all the animals in latter experiments were placed under anæsthesia by himself—the longest lasted about six or seven hours—all were killed after those experiments except three, which were kept alive for observation—it was supposed they would remain insensible to pain—at a medical *conversazione* the experiments were repeated, and Dr. Ferrier's discovery was demonstrated—no pain was given to a single animal at these, which were the first series of Dr. Ferrier's experiments—Mr. Jesse's remarks in *The Times*, and in other papers on this subject, were utterly unfounded—witness has a private laboratory—he would like the protection of the law for experimentation.

DAVID FERRIER, M.D.—Had performed many experiments, generally under anæsthesia, and always, when possible, before students, it is unnecessary to cause pain to animals except in the administration of poisons—he thinks it necessary for students to see the latter—these rarely give much pain—painful experiments for research are absolutely necessary and justifiable—anæsthesia is produced in the majority of experiments—at present, theoretically, any man may perform experiments; practically few do; and no law should be allowed to prevent a competent man from experimenting as he desired—he would object to inspection of any kind in his experiments—he would not object to a license being given to protect the operator, without compelling him to submit to inspection or to register his experiments—original research ought to be encouraged and not restricted—he would like legislation for the purpose of declaring experiments for research to be legal, and thus to quiet the public mind—he does not know what the Secretary of the R.S.P.C.A. meant in his evidence about the levity at his lectures at the London Institution—he does not feel that there was anything laughable in his remarks, and there was no levity—he thinks he told his audience that the animals he alluded to were

insensible to pain during the operations—the Secretary's impression was unfair—the character of experiments may have seemed “grim” to the Secretary, but he did not lecture with any degree of humour, which he could not claim credit for, even to cover the “grim” character of his experiments—he did not laugh himself, and had no desire to be sensational—the lecture was delivered before a mixed popular audience of about 300 persons, the great majority being non-medical persons, and it was essentially the same as that he delivered at Bradford—he is not conscious of having gesticulated, and had no intention to do so—there is not a shadow of foundation for the secretary's statement that he “made his audience laugh over the grim behaviour of his unfortunate victims”^{*}—he must again express great disapprobation of the proposal to inspect laboratories, even should the inspector be a competent scientific man, but if a layman, like the secretary, he would view the prospect with feelings of apprehension—he would also object to places being licensed, and would “allow everybody liberty to perform experiments in his own private laboratory”—there are a great many experimenters who live in the country—he experiments at his own house as well as at King's College—he would disbelieve the statement of Sir William Fergusson, that animals are crucified on boards—also respecting the “maleficent tendency” of students' minds, as many of them belong to Christian Young Men's Associations—he had never seen any “frightful” experiments of any kind—he had no recollection of saying at his lecture that he was afraid to say on how many cats he had made the same experiment and he saw no laughter at the lecture†—during his experiments alluded to he had seen no indications of pain—referred by Mr. Hutton to his own description of one experiment as follows: “I was rather inclined to attribute the phenomena of barking to retention of consciousness and distinct sense of pain,” he said he had forgotten that circumstance during his evidence—referred also to the sentence, “the animal was only partially insensible,” he said it was “sufficiently narcotised to abolish any real pain”—referred also to the sentence, “the animal exhibits signs of pain, screams and kicks with both legs,” he said the words “signs of pain” meant reflex actions only—referred to the sentence, “long-continued cries as if of rage and pain,” he said the qualification of the word “if” shows he did not believe in the reality of pain—he did not know why he qualified the last sentence and not the others, but if he had conveyed the impression that pain was felt he did not intend it, except in the instance when the dog barked—referred to the sentence which alludes to a dog howling and barking, “apparently it remained conscious, for when called it would struggle to get up, and would sometimes regain its feet, and even succeed in walking a few

* As to this contradiction see newspaper report, &c., pp. xlv. and xlvii.

† See Mr. Sawyer's evidence

steps when it would fall over in a helpless manner," he said there is nothing there to indicate suffering—he would rather allow a psychologist who might be a bad physiologist to experiment in a way that might torture an animal than sacrifice the psychological results obtained by experiments—he experiments with strychnine on rats, mice, birds, rabbits, and guinea-pigs—barking is not an indication of pain—frogs croak and dogs bark without pain—he would not interfere with experiments which, to a contemporary, might appear trifling even.

GEORGE HOGGAN, M.B.—(Secretary to the Society for the Protection of Animals liable to Vivisection.)—Vivisection ought to be permitted under conditions—experiments may be made amenable to public opinion—all secret painful experiments should be forbidden—all painful experiments should be conducted in a "suitable Hall fitted with the necessary tables and apparatus" for practising vivisection, "overlooked by a gallery into which the public could have unrestrained access"—thus the operator would not be interfered with—the public could see and estimate the nature of the experiments; guilty operators could be prosecuted; experimenters would be on good behaviour; any experimenter, whether with a special license or not, could demand convenience to experiment; students might be admitted as spectators to "watch every step in the operation" which is of "primary importance;" any spectator in the gallery being a student might be called down to assist in the experiment; all admissions to the gallery gratis; programme notes of the experiments to be hung in the gallery; experimenters desiring to conceal the nature of their operations would be prevented; inoculated animals would be retained on the premises and be accessible to the public, those suffering from painful wounds to be destroyed; any person or corporate body to be at liberty to erect such a place, which, after inspection by a government officer, should be licensed to the owner giving him power of management; all wounding of animals causing much pain to be done under anæsthesia (this to refer only to the cutting process, not during the entire experiment) curare not to be considered an anæsthetic—horses, monkeys, dogs, and cats, not to be experimented on by any cutting process—his proposal would make "vivisection as easy as possible when it is conducted in a proper way"—his plan had been approved by a considerable number of other men, whose names he is not authorised to give—a repetition of classic experiments is an abuse (examples quoted)—vivisection is the same all the world over—he had seen only a little in this country—English experiments must necessarily be the same as continental—the only experiment he had seen in England was on a frog; but he had read experiments (quoted) recently performed by Dr. Rutherford on dogs which were totally unnecessary and unproductive, and cruel in the extreme; doctors differ on these cruel experiments but agree that what is applicable to the dog is not applicable to man—

had not seen those experiments—pressed to state what he had seen in England : he said strychnia given to a dog by a silly person and a galvanic battery applied ; the dog died, and the result was useless ; he had seen nothing else except his own experiment which failed at the stage of administering an anæsthetic—he was strongly urged to try experiments by his teacher at Edinburgh, and refused—his teacher, Dr. Handyside, offered to associate himself with him—he was a student at this time—he was advised by several other teachers and many medical men and students to experiment—the same advice was no doubt given to other students—anæsthesia was not mentioned—he believed there was much private experimenting amongst students without anæsthetics—one gentleman, Dr. Alexander Sinclair, advised him not to have cats, because they “make such a squalling and scratching when you begin to cut them”—witness had retired from the navy, and made medicine a sort of pastime—[Examined further on a subsequent day]—he said when he objected to experiment at Edinburgh because of the pain he would give, the answer he must admit was sometimes made “cannot you give them anæsthetics?”—the classic experiment he quoted on the last occasion was a most valuable experiment but should not be repeated—he would not put down vivisection under proper restriction, but if physiologists would not allow such restrictions then he would advocate total abolition—certain humane persons in speaking of vivisectors are guilty of exaggeration, representing them as though “flourishing knives and red hot irons over their victims ;” such erroneous impressions would be removed if those persons were allowed to witness experiments—he is conscious of the awkwardness of his plan as regards physiologists, but the animals and the public must be considered also—he is not acquainted with any London laboratory—at Edinburgh it was thought that students might perform experiments at the university—an experiment (described and put in) performed by Paul Bert, in France, was very painful, lasting for ten consecutive hours, the only result of which was a discovery that when tortured the animal urinated ; the poor creature was left in pain all night under curare—curare is not an anæsthetic as proved by the best physiologists, Mr. Yule’s experiments notwithstanding—there is a great difference between narcotics and anæsthetics—contrary opinions exist among experimenters respecting tuberculosis, and the whole field still lies open for experiments—all experiments, up to the present, have only shown what tuberculosis is not—we have no better knowledge now than we had thirty years ago—questioned as to Paul Bert’s experiment he would not admit little pain only was caused—in a paralysed man’s body, without sensation in his feet, he knows the feet would be drawn up when the soles were tickled—he does not know that such impressions are conveyed through the sensory nerves, and would rather not give a hasty opinion on a subject that requires thinking about ; even if that be a statement accepted by every physiologist in the world during the last twenty

years—he could not say that such consequences result from reflex action—he objected to experiments at Edinburgh because of the pain he would give animals in his operations, but went to the Continent for the purpose of getting a knowledge of experimental physiology, which he was unable to procure at Edinburgh—he knew that experiments were tried at Edinburgh by students, because they said, “I have tried so and so”—he could not say where they were performed—he could not name any of the persons—he thought one student said he had operated on fifteen cats; he knew that student, and that he was assisted by a fellow student; one of these is now dead, and he does not know the name of the other—he cannot give any other particular instance—he cannot give the name of any other student in relation to such experiments—but at the time these practices were openly spoken of—he cannot now give further information, and the Commission would not be able to get it; they may as well try to inquire into Freemasonry—Claude Bernard, the greatest vivisector of France, has recently said that vivisection is not sufficiently checked among incompetent experimenters, and he deprecates the excessive use of curare—physiologists are not the monsters that some people picture them—among them may be found the kindest, most amiable, and most gentlemanly of men; they do not gloat on torture; but are unconscious that they inflict pain.

EMANUEL KLEIN, M.D.—He is the author of the first part of the *Handbook for the Physiological Laboratory*, many of the experiments in which are necessarily painful—no directions were given for anæsthetics because the rules in that respect are understood by students in a laboratory—in his own experiments he uses anæsthetics only for convenience, and entirely without regard to the sufferings of the animals—an experimenter has no time to think whether the animal will feel pain or not—in demonstrations he uses anæsthetics because the feeling of Englishmen is against painful experiments, and as a foreigner he knows that such feelings ought to be regarded—he had practised in Vienna, and generally on the continent he believes that experimenters altogether disregard the feelings of animals—there is a great difference between them and Englishmen, except a few physiologists here—dogs are chloroformed for convenience, to prevent their howling, and cats to prevent their scratching—sportsmen do not regard the feelings of the animals they hunt—there are no written rules in laboratories to direct students when to use anæsthetics—there is more experimenting in the country now than when he first arrived in England—there is no private experimentation except in a laboratory, he believes, but there is some at the Brown Institution by students who act under him or Dr. Sanderson—the animals are bought for experiment there—he experiments for the Privy Council under Mr. Simon—frogs are imported from Holland for experimentation—when dogs are operated upon they are fastened down by broad

bands, their limbs being extended and secured—when he finds inconvenience from the cries of the animal he uses chloroform—as rabbits do not howl nor scratch he never anæsthetises them—his private experiments at the Brown Institution are performed in his own room: he lives there—he holds no appointment, but is called assistant professor, and the gentlemen who come to the Institution to make special investigations, work under his instructions—he does not think his experiments are for the Brown Institution, but he believes they are for the Privy Council—his experiments are very painful, producing artificial tuberculosis—some of the pathological experiments carried on for the Privy Council are particularly painful in their consequences, viz., the injection of fluids into the abdominal cavity or veins—sometimes the animals remain for weeks affected by such injections—about ten or twelve animals are painfully operated on by him at the Brown Institution every year for the instruction of his own pupils—a physiologist has a right to do as he likes with an animal—he has performed upon guinea-pigs, rabbits, rats, mice, frogs, dogs, cats, monkeys, and sheep, for special research—he has performed the experiment on the mesentery of a frog mentioned in the *Hand-book for the Physiological Laboratory* before his pupils, and several times for pathological investigation—foreign physiologists agree with him that the suffering of animals should not be taken into account at all, except when it interferes with their convenience—Burdon-Sanderson does not agree in that view—he has received no minute from Mr. Simon, written by Mr. Forster, to prevent painful operations and to secure a return of experiments made on behalf of the Privy Council. [This answer was given to Mr. Forster, who quoted from his own minute alluded to.]

EDWARD ALBERT SCHAEFER, M.R.C.S.—At University College the treatment of animals is most strictly dictated by humanity—practical physiology is taught there to students by the Professor, Dr. Burdon-Sanderson, who experiments on living animals, and shows such experiments; also in another department just springing up, advanced students and medical men perform experiments in the laboratory under the professor or under himself—anæsthetics are seldom used by him in operations on frogs, as he believes those animals are not very sensitive to pain—he studied abroad—if frogs were sensitive the experiments would be very painful—sometimes frogs are decapitated, and sometimes pithed, but only occasionally—he knows very much of London students, and is assured that they do not experiment except at the laboratories of University College, King's College, Guy's Hospital, and Brown Institution—he never heard of their experimenting in their own rooms, and never heard of a “frightful” experiment before them or by them, as anæsthesia is always present.

JOHN GRAY MCKENDRICK, M.D.—Alluding to the experiments

of Dr. Bennett (see p. 20, No. 43), in which he assisted generally, a great many observations were made without, he is sorry to say, finding a sufficient antidote for opium, but an antidote for strychnine was discovered, viz. :—hydrate of chloral, by which already the life of one man at least has been saved—it is not necessary to repeat those experiments, which were painful—anæsthetics ought to be used in demonstrations to pupils, and painful experiments are very seldom required at all—he had discovered a new anæsthetic by means of experiments, viz. :—hydro-chlorate of chinoline, which may be used in all experiments on frogs—he tried it on himself, which caused violent vomiting—he believes that frogs are capable of suffering, and he therefore treats them in operations on that hypothesis, saving them from as much pain as possible—he has seen much of students, but has never heard of their experimenting, except in one instance, when the student was stopped—vivisection has advanced physiology by discoveries by Sir Charles Bell, on the nerves; by further discoveries on the nerves by Brown-Séquard, Dr. Reid, Dupuy, Pourfour du Petit, Brachet, Marshall Hall, and others; on the functions of the cerebellum, by Flourens and others; ditto on the surface of the cerebral hemispheres and the motor functions of grey matter by Flourens, Majendie, Hitzig, Fritsch, Ferrier, Gudden, and Nothnagel; by demonstrations on the circulation of the blood by Harvey, and other phenomena relating to the circulation by Hales and Ludwig; the action of air on animals by Robert Boyle and Mayo; facts regarding respiration by Priestley and Lavoisier; facts relating to digestion by Blondlot, Schwame, Bernard, Lehmann, and others; facts relating to the functions of the lacteals by Colin, Bernard, Ludwig, and others; facts on the action of light on the retina by Holmgren, Dewar, and McKendrick; on the liver, by Bernard, Macdonnell, Pavy, and others; and relating to electro-physiology by Matteucci, Du Bois-Reymond, Pflüger, and many others—discoveries in aiding medicine and surgery have also been discovered by vivisection in the transfusion of blood, artificial respiration, the causes of the cardiac sounds of the heart, the circulation within the cranium, aneurism by Hunter, relating to the regeneration of bone, diseases of cartilage and of inflammation, and zymotic diseases—experiments have also led to the advancement of therapeutics, and to the relief of pain, &c., viz., the use of ether, chloroform, and chloral; action of Calabar bean, and antagonism between active substances with antidotes—to record all facts given to physiology would be to write the history of science, and to arrest experimentation would stop the progress of physiology, pathology, and therapeutics—he does not think that legislation would have any distinct beneficial effect, but he has no objection to licensing—he has a private laboratory, and would object being compelled to go to a public one on all occasions.

ROBERT SAWYER (Barrister-at-law).—He was present at a lecture

by Professor Ferrier, at the London Institution, which was certainly very interesting—in reference to one of his experiments Dr. Ferrier remarked “I am afraid to say how many cats I have operated upon in this investigation”—there was a titter amongst the people and an expression of feeling which he was disappointed to see—he was pained at the fact that the people seemed to look upon that remark with considerable levity—Dr. Ferrier did not mean to promote indifference to pain, but probably considered himself justified in taking away the lives of any number of animals for investigation with indifference, but he supposed he would say the animals did not suffer pain—he understood him to say that at the commencement of his lecture, but he said afterwards that there were indications of feeling certainly—if the lecturer did not laugh himself, he smiled—he could not speak of all that took place because there were two lectures, and he attended only on the first occasion, and did not hear the second lecture—possibly therefore Mr. Colam’s evidence might refer to what took place at the second lecture.

JOSEPH LISTER, M.B.—Experiments are the most important means for promoting surgical and medical knowledge—the majority may be performed under anæsthetics—operators ought to be competent—if legislation do not hamper investigation he will not oppose it, but he thinks it uncalled for—his father-in-law, the late Mr. Syme disapproved of needless experiments, but highly valued vivisection—he had shown experiments under anæsthesia to pupils, the effect being beneficial—pain ought not to be caused to animals without a sense of necessity—there are stronger reasons for vivisection than for the castration of animals—it would be highly demoralizing to students to witness unnecessary pain—the total pain caused by vivisection in England in one year does not amount to that inflicted during a single day by the winging of pheasants—he has himself experimented at very great sacrifice to his feelings, but he puts probable good to humanity into higher consideration than his own feelings—demonstrations by painless experiments to students are invaluable—the proposal to erect galleries in laboratories for the admission of the public would be highly improper, as a public spectacle should be prevented on moral grounds, and painful experiments should not be retarded by the presence of strangers—he has made important discoveries himself by experiment—there is a close analogy between the larger animals and man—a frog is not capable of so much pain as higher animals—medical men are the most humane, which may arise from their being constantly engaged in investigating suffering—physiologists without medical practice are not under the same humanising influence—physiologists are increasing in number—he does not think it necessary to give anæsthetics to frogs—the scream of a rabbit is not a pleasant thing to hear—it would be very undesirable to make any exemption from experiment of the higher quadrupeds.

ROBERT McDONNELL, M.D.—The public feeling in Ireland and of students is against painful experiments—few painful experiments are needed—protracted pain ought to be prevented—there has been great exaggeration on this subject, which legislation might correct by disarming the suspicions of the public—legislation might also prevent experiments by incompetent persons—there are no schools in Ireland where painful experimentation is used for instruction—in his own laboratory experiments, some of which have been performed before some of his senior students, the pain of cutting has been prevented by anæsthetics, but slight pain subsequently followed the return of sensibility after the completion of the operation—experiments are of essential importance in the promotion of medical science—he never knew a medical student think of experimenting—the erection of a gallery in laboratories would be ridiculous, vexatious and mischievous—experiments have tended much to the mitigation of human suffering—he constantly uses chloroform on frogs and tadpoles—the process is not difficult—he strongly recommends it.

THOMAS HAYDEN (Professor).—He had formerly performed experiments before his class, but not during several years past—experiments are not necessary for teaching purposes, and students do not generally profit by them when they are performed—for investigation experiments are necessary, but within limits, and for new discovery.

JOHN CLELAND, M.D., (Professor).—He never exhibits animals higher than frogs to his pupils under experimentation, and has never thought of applying anæsthesia to a frog—frogs are generally pithed or decapitated—experiments are necessary in teaching—no person he had met with would omit using anæsthetics when possible—if he were teaching students practical physiology for the purpose of making them thorough physiologists he would use the *Handbook for the Physiological Laboratory* and perform the experiments mentioned there—he would not scruple to show painful experiments to students.

CHARLES DARWIN—He approves of the resolutions of the British Association (p. 150), and was very glad to see them—he had never practised experiments on living animals—progress in physiology cannot be made without vivisection, and physiology is destined to confer the highest benefits on mankind—experiments may generally be made without pain—it was unreasonable to object to painless experiments—a Hindoo who objects to animal food might object, but English people cannot do so logically—unnecessary painful experimentation deserves detestation and abhorrence.

FRANCIS SIBSON, M.D.—Experiments are necessary for the progress of medicine—if they are stopped the wings of science will be clipped—physiology is now taught in a totally different and much better

manner than it was formerly—the old style can scarcely be called teaching at all—physiology is the foundation of medical treatment, and it cannot be taught without experiments—his experiments have been of the greatest service to him in the discovery of disease—legislation would be a great evil if it impeded the progress of physiology—all experimenters known to him are guided by humanity and kindness, and he knows no one capable of omitting anæsthetics where they can be applied, or using them only for the sake of convenience—that is utterly unknown to physiologists in this country—it is untrue to say that experiments become attractive to medical students in proportion as they are “frightful,” for students are repulsed by the sight of pain—Goltz’s experiment of boiling a frog to death is horrible—curare in his opinion produces insensibility to pain—if curare has been tried on man and has shown opposite effects he would decline to accept such conclusions—even if curare be not an anæsthetic, but simply paralyses the motor nerves, then that which is called a painful operation, lasting eight hours and a half, would not, he thought, be very painful, because the animal would be motionless, it would not struggle, the incisions could be made with ease, and the animal’s mind would be withdrawn from the domain of attention; for “when there is no attention there is no sensation, no sensitiveness, no pain”—there is very little suffering caused by the starvation of animals—before death the temperature of the animal lowers so much as to produce anæsthesia—pressed very much, he might add, that in the earlier stages of starvation he would not say so; he does not know—there must be some discomfort—he is not aware that human beings feel something more than discomfort when starved—reminded of the sufferings of sailors when exposed to the process of starvation, which sometimes leads them to cannibalism, he might feel bound to admit that such sufferings amounted to more than discomfort, but pain ceases after awhile when cold acts upon the nerves.

JOHN MALLET PURSER M.D., (Professor).—As a teacher of physiology in Trinity College, Dublin, he had felt much fettered by not being allowed to perform experiments on animals before students—he would like to perform experiments on warm blooded animals, even not made insensible to pain—the teacher should be entirely unfettered, and not amenable to the sentiment of humanity on the part of the public—even if the Professor performed ill-judged and unnecessarily cruel experiments, the legislature ought not to interfere.

JAMES B. MILLS, M.R.C.V.S.—He had been desired to appear as a witness by the Secretary of the R.S.P.C.A., to whom he gave a written statement relating to experiments performed by him and other students at Edinburgh (see page xlii. for statement in full)—such experiments he has no doubt will be repeated, if not stopped—animals were used for demonstration at the new Veterinary College

before death and dissection—chloroform was always used, but when the animal was not killed it suffered much pain for twelve or fourteen hours after sensibility had returned—this took place with the knowledge of Principal Williams—a dog was brought to the College to be destroyed; prussic acid was given to it in the surgery, but when brought out it did not die; students then dragged it to the dissecting room and gave it ammonia, with a view of restoring it; the antidote failed, and then one of the students knocked its brains out—these things took place before he became lecturer on *Materia Medica* at the same College—during the winter Session of 1874-5 he had been informed that a horse bought for dissection was kept alive for more than a week at the same College for experiments—the students performed several experiments on it, consisting of neurotomy, tenotomy, venesection, and other operations—between the operations the animal was turned out into a paddock, and after a week's sufferings it was killed for dissection—he was not present during that Session, but had been told by a student the full particulars of those experiments, conducted in the College grounds—anæsthetics were not used—the animal was cast by means of hobbles—the Principal must have been aware of the experiments which were not conducted secretly; the animal was exposed within the precincts of the College, in an emaciated condition—operations were made on various parts of its body.

WILLIAM BENJAMIN ARCHIBALD SCOTT, M.D.—He saw an experiment in the University of Edinburgh in 1871 under curare which disgusted him, and prevented his further attendance at the class. He referred to the "Handbook" for several different experiments on frogs which he contended were of a painful character—he had never known an operation cause abhorrence to medical students—students performed experiments on frogs in their own rooms at Edinburgh, and he believes in London also—he has no personal knowledge of this—they were not on cats and dogs, but only on frogs—he cannot give the name of any student in England or Scotland who has experimented privately in his own room, but he is sure that the practice is common wherever students are zealous at any College.

WICKHAM LEGG, M.D.—He had performed experiments on animals at St. Bartholomew's Hospital, London, altogether consisting of four or five series—he performed on sixteen cats, some of which lingered eight, ten, seventeen and twenty-three days (see p. 70, No. 116)—the animals suffered very little pain after the bile ducts had been ligatured—he had performed other experiments relating to fever and the action of a new drug, on the inoculation of cancer, and to ascertain the cause of the appearance of bile pigments in the urine—experiments are useful and indispensable—in pathological cases you can kill an animal and examine the process of the disorder, which cannot be done in man—artificially produced disease is the same as natural disease

and the pain caused would not affect the condition of the patient—all his operations had been performed under anæsthetics and treated with as much tenderness as the experiment permitted.

ARTHUR GAMGEE, M.D., (Professor).—Experiments are absolutely necessary for science and medicine, and have established the most valuable facts—his experiments are performed at Owen's College, Manchester—if for demonstration to medical pupils the animals are invariably narcotised and destroyed before recovery can take place—since his appointment no pain has been inflicted on an animal for demonstration—without exception students may be taught all that is necessary without animal suffering—for original research painful experiments are necessary sometimes—these require instruments of great delicacy the management of which is possible only by properly instructed experts—persons without such knowledge are not likely to advance science by private research—vivisection in England has not been practised with abuses—students need not witness painful demonstrations on the actions of poisons—curare is not a safe anæsthetic as its effect is doubtful though it may produce a species of insensibility—he had tried it in practice on a human being in the treatment of nervous affections when sensibility was not at all impaired—in his lectures to students all operations on frogs even are performed under anæsthesia or when the animal has been decapitated and its spinal cord destroyed—a watery solution of chloroform is an admirable method of rendering frogs rapidly insensible to pain—he saw no objection to restrictions being placed on incompetent experimenters.

GEORGE JAMES ALLMAN, M.D., (Professor).—Experiments are necessary and may be made almost entirely painless—abuses should be put down—Dr. Lyon Playfair's Bill was good on the whole, much better than Lord Hartismere's—for the purpose of teaching there is no necessity to perform painful experiments—not even with regard to the administration of strychnia as the tetanic results of strychnia are plain and intelligible—all experiments should be registered and legislation is desirable even to the appointment of a Board of Control.

SIR WILLIAM WITHEY GULL, BART., M.D.—He thinks the present excitement on this subject has been caused by the publication of reports of foreign experiments—experimenters in this country are humane—vivisection is absolutely necessary—students do not show indifference to pain—he denies that students are attracted by sights of suffering or animal torture—to say so is a serious libel on medical students—operators are aware that suffering complicates and prevents results and therefore they endeavour to prevent it—he would regard legislation as a great evil—students ought not to perform experiments

but should see them performed—Englishmen are naturally humane—he does not believe that the R.S.P.C.A. has done any great service in preventing cruelty—legislation would not be necessary, even if it were proved that operators never use anæsthetics when they might do so except to suit their convenience—so long as you may take an unwilling man and slaughter him in war you may be allowed to take animals in vivisection, but an animal should not be wantonly tortured—science justifies itself—it is unnecessary to give strychnine to animals for the teaching of students, but he would not object to that being done by high-minded teachers, or even the most painful experiments on the sensory nerves, or any other experiment if the teacher thought it necessary to show the same to students—investigators should not be touched at all, not even such as are perfectly indifferent to the sufferings of animals—the advantages to medical practice arising from vivisections bristle around everywhere, relating to dropsy, the liver, apoplexy, etc., etc., the difficulty he feels being to know where the list would end if made out—vaccination for instance came from experiments, and vaccination has saved more lives than the Great Napoleon ever destroyed in all his wars.

WILLIAM BENJAMIN CARPENTER, C.B., M.D.—He would approve a law on this subject if it could be adopted without interfering with the progress of physiological science, but there is great difficulty in details which might prevent a man performing certain experiments in his own house which sensible people ought not to oppose—vivisection, he has no hesitation in saying, is of essential importance to physiology—so long as Sir Charles Bell experimented he achieved great results, but in trusting too much to anatomy he was led into mistakes which had to be corrected by vivisections—painful demonstrations before students ought not to be allowed; these are not only unnecessary, but morally injurious—painless experiments are of great advantage in teaching students, and, morally, we have as much right to “cut off the head of a turtle to exhibit the great phenomena of reflex action upon upon its dead body, as we have to kill it for food”—he would justify the most painful experiment during investigation if absolutely necessary for an important result—Dr. Brown-Séquard is a most humane man, but he had inflicted more animal suffering probably than any other man of his time; and his labours have been richly productive of good—by paralyzing animals and carefully nursing them through months of suffering, he proved that the spinal cord may be restored, and the power of the limbs regained—those experiments need not be repeated—before the introduction of anæsthetics he had seen a perfect callousness to animal suffering when important experiments were being performed—as regards English physiologists at the present he believes they are anxious to prevent pain.

THOMAS LAUDER BRUNTON, M.D., D.SC.—His experiments on

animals are generally performed under anæsthesia—he prevents as much suffering as possible—several new remedies have been discovered by vivisection during late years, viz:—nitrite of amyl to relieve *angina pectoris*, chloral, atrophia to cure salivation, apomorphia as an emetic, and Calabar bean into medicine, and Claude Bernard's discovery respecting the method by which carbonic oxide produces poisoning—the latter discovery has recently saved a man's life—the most painful of his experiments is that of poison by snake bites, in which anæsthetics cannot be used, and the animals suffer for about half an hour—perhaps poisoning by strychnia is equally painful—it is quite the exception for him to give any pain—curare is an anæsthetic to a certain extent, but there is no certainty in the matter—it paralyses the sensory nerves in frogs as well as the motor—Dr. Moore prepared the return sent in from St. Bartholomew's Hospital to the Royal Commission during his absence (see page 151) which was incorrect; it should have stated anæsthetics are invariably used when possible—experiments in demonstration before students are necessary, but they may be made almost entirely painless—for private research the number of animals he employs varies enormously, ninety being the largest number in a single series of experiments, and those were cats placed under chloroform; but they were allowed to live four or five hours after anæsthesia ceased, in trifling pain—those experiments have yielded nothing at present but they are not finished—cats are selected because they are good for experimentation and cheaper than rabbits—if operations are done rightly on cats results are certain—dogs cannot be got—cats are supplied by a man—no inquiries are made where they come from—he has never found any difficulty in anæsthetising frogs—the ninety cats alluded to were his first series of experiments relating to cholera—he had performed three series—he had experimented for other purposes on rabbits, guinea pigs, dogs, frogs, pigeons, and fowls—for snake poison injection perhaps on about 150—he studied in a German laboratory—all his experiments on the liver had been done under chloroform—there is thorough insensibility to pain when animals are well narcotised with morphia or opium—if anyone has stated the contrary he would not hesitate to contradict such statement emphatically however high the authority might be—it is contrary to the truth to say that narcotised animals are still sensible to pain—experiments are absolutely necessary for the discovery of antidotes for snake bites.

ALEXANDER JAMES SINCLAIR, M.D.—His attention having been called to Dr. Hoggan's evidence, in which he implied that he (witness) was in the habit of advising him to operate, and he (witness) operated without anæsthetics, he would say such statement is not correct—he must also negative Dr. Hoggan's other statement respecting the squalling and scratching of cats when operated upon (see p. 177)—he had stood to Dr. Hoggan in the relation of teacher—the opinion of

the University would strongly condemn any man for not using anæsthetics when possible—he would desire inspection if it were to prevent incompetent persons from operating, and other physiologists would.

PETER DAVID HANDYSIDE, M.D., F.R.S.E.—His attention having been called to the evidence referring to himself given by Dr. Hoggan, (see p. 177) in contradiction to some parts of the same he would desire to say that it is not true that it is common for medical students to try private experiments with the concurrence of the lecturer at Edinburgh—he believes that no lecturer ever did such a thing—he would not allow any one not acquainted with anatomy and physiology to experiment on a living animal—anæsthetics, so far as he knew, are always used at Edinburgh when possible—it would not be possible for a man to say in Edinburgh medical society that he was totally indifferent to the suffering of an animal during an operation, and used anæsthetics only for convenience—such a monster he had never met—it is not true that he had any intention to associate himself with Dr. Hoggan, as stated, in the experiments—he had been teacher during twenty-six years at the School of Medicine—it is not necessary to demonstrate to classes with living animals—he believes other teachers like himself are anxious to avoid suffering to animals—experiments are the basis of medicine—he would refer to experiments made by Marshall Hall, Astley Cooper, Barry, Syme, and Brodie, as having been essential to different phases of disease—he had never seen experiments on frogs at Edinburgh producing pain—he was the first honorary secretary to the Scottish Society for the Prevention of Cruelty to Animals, and his labours in that field might be taken as evidence of his regard for animals—he thought it right that the public should have ample security that vivisection is not improperly carried out—vivisection should be licensed and inspected by an expert, and by two laymen to represent the public—he believed the profession would not object—he had heard opinions to that effect—every possible check should be made against cruelty.

WILLIAM WILLIAMS, V.S. (Professor)—His attention having been called to the evidence given by Mr. Mills (see p. 183), he must admit that the experiments took place, but entirely without his knowledge—it was an abuse and the only one he ever heard of at the New Veterinary College—with regard to the other statements made by Mr. Mills, they are not exact and convey a wrong impression—horses are bought for dissection, and operations are performed on them not for cure but for experiment and under chloroform—he had never performed an experiment on a cat or dog in his life, and he believes students at the college do not—students are allowed to bleed animals at the College whenever they please: that entails no suffering and makes students proficient—there is no superintendence over such bleeding—he was absent during the time when the abuse alluded to took place—he left his

assistants in charge ; they did not discover the abuse at the time, and neither he nor they heard anything about it until Mr. Mills appeared before the Commission ; when he heard of it he inquired of a groom who confirmed the statement—he thought it possible that rules could be made to prevent the recurrence of such abuse.

EDWARD CRISP, M.D.—He is a penitent, having vivisected animals for many years, and now in advanced age he regrets it ; but he should be sorry to see vivisection done away with altogether, for he believes that no medical man had ever experimented without a good motive.

JAMES MADEN HOLT, M.P.—Is a member of the Committee of the Society for the Abolition of Vivisection, but not well acquainted with its proceedings—had never attended a meeting—the organisation of the society was not satisfactory—he has quite made up his mind to retire from the Committee unless some change be made—Mr. Jesse is the secretary and treasurer—he knows of no meeting having been held for the purpose of embodying the opinions of the society on this question, nor of the Committee—Mr. Jesse has received no authority to speak in the name of the society, or to express its opinions—Mr. Jesse's action has been entirely his own affair even to the writing of letters to the Commission in the name of the society—he is not aware that Mr. Jesse has any sanction for the issue of printed documents or papers—he (witness) is opposed to all forms of Vivisection—he read quotations from Dr. Macaulay's *Plea for Mercy to Animals*, and works by Robert Boyle and others, quoted elsewhere—after stating several objections he begged the Commission to abolish the practice altogether and forbid it under heavy penalties—he is aware that the R.S.P.C.A. have given their support to a bill which abolishes all painful experiments, but he would go beyond that—if it could be proved that vivisection could be conducted without pain he should still oppose it—he never would believe in the humanity of vivisectors to apply anæsthesia—they would break the law compelling them to anæsthetise animals—if a law were passed to prohibit all vivisections they might break that law also—he knows that certain pain is inflicted on animals for human food and the good of the community, the abolition of which would require some consideration as that might be necessary, but vivisection is not necessary—he entirely objects to obtaining amusement at the expense of animal suffering—he is not a sportsman—there must be pain caused in the taking away of animal life—he should look with less aversion on a conscientious vivisector than on a sportsman who selfishly amused himself at the cost of animal pain—referred to Harvey's own words on his discovery as opposed to Robert Boyle's remarks, he must admit that Harvey knew his own mind better than anyone else.

GEORGE HENRY LEWES.—Vivisection is indispensable and a painful necessity, and experimenters must be left to determine the experiments necessary to be performed—a great deal of experiment is useless being too complex—[he cited different instances]—experimenters try to prove that which cannot be proved—in reference to the innervation of the heart he scarcely knew a single statement which had not been contradicted by somebody else—one man discovers a fact by experiment and instantly all over Europe certain experimenters set to work to repeat it scores of times—if properly done an experiment need not be repeated but only confirmed—he had studied entirely in foreign laboratories—experimenters are morally bound to prevent unnecessary experimentation—he is not a medical man but a physiologist and had experimented very much—students generally are too indifferent or too indolent to experiment—he would like to restrict experimentation if possible—pain to animals is in the “wounding, not in the *wound*,” and the wounding is almost always done under chloroform—it is a cruel thing even to disable an animal, which should be avoided if possible—there are very few people performing experiments in England—the number of thoughtful experimenters ought to be increased—he performs principally on frogs, about 90 per cent. of which are etherised—he cannot bear to perform experiments on cats or dogs but he had experimented on rabbits and pigeons—he always uses an anæsthetic and kills the animal before sensibility returns—he has experimented in most of the laboratories of Europe—there is very great difference between the feelings of operators abroad and those in England with regard to causing pain to animals; and the French and Italians are much more indifferent than the Germans are; strikingly so—in England humane feeling is still more apparent—two of the greatest experimenters in Europe use anæsthetics in order to attain perfection in the experiment.

GEORGE RICHARD JESSE (Secretary to the Society for the Abolition of Vivisection).—This gentleman appeared on three different occasions before the Royal Commission. Having stated that he appeared as the Society’s representative and at the wish of the Society, he was severely cross-examined as to his credentials, and many pages of the Blue Books are filled with questions and answers on these points, and on the relevancy of his statements. The Royal Commission have published only a portion of his evidence, which consists of his own opinions based on passages referred to in books. An analysis of such evidence cannot therefore be given here with justice to Mr. Jesse, we regret to say; but this loss will be repaired by the publication of his statements in full, the issue of which has been announced; and the reader is therefore referred to Mr. Jesse’s own report.

REPORT OF THE ROYAL COMMISSION.

TO THE QUEEN'S MOST EXCELLENT MAJESTY.

IN obedience to Your Majesty's commands, we have inquired into the extent to which the practice of making experiments upon living animals,—with a view to the advancement of science, or to the acquisition of knowledge available for the relief of human suffering or for the prolongation of human life,—is carried in this country,—and we have considered whether any and what measures ought to be adopted in respect of that practice. We have had the advantage of examining many eminent physicians and surgeons and physiologists of great reputation, as well as of hearing from the Secretary of the Royal Society for the Prevention of Cruelty to Animals the views of that Society. He has also laid before us several documents relating to the subject, which we have printed in the Appendix. We have thought it sufficient to give the references to that portion of them which consists of opinions extracted from published works or journals.

A new Society has been formed under the name of the Society for the Abolition of Vivisection. A letter from Sir George Duckett, Bart., whose name is at the head of the list of the Committee, declining to give evidence before us, will be found in the Appendix. Mr. Holt, M.P., for the North-eastern Division of the County of Lancaster, one of the members of the Committee, has given evidence in compliance with our request. He stated to us very clearly and fully the conclusions at which he has arrived and his reasons for them. Some observations which he made to us on the constitution and course of proceeding of the Society will be found in the Minutes. The honorary Secretary gave evidence at considerable length. It consisted in great part of extracts from published works several of which we have not reprinted in extenso in the minutes,—but we have given the references so as to enable those who may desire it to refer to the originals for themselves.

Dr. Emanuel Klein, assistant professor at the laboratory of the Brown Institution, lecturer on general Histology at St. Bartholomew's Hospital, gave evidence before us. Dr. Klein has acted in the investigations which have been conducted under the medical officer of the Privy Council, and is author of the first part of the handbook to which we shall have to refer. The proof of his evidence was sent to him in the usual course for his corrections. This he returned with alterations which appeared to us to be so much at variance with the letter and the spirit of the answers that he had given us at his examination, that we felt ourselves unable to receive

them as an authentic report of his evidence. In consequence of this refusal he has requested permission to withdraw the evidence. We have thought that this course would not be right, and we have included in the minutes the shorthand writer's note as it was originally taken, and have given in the Appendix the amended proof submitted to us, with the correspondence on the subject.

We find that until a comparatively recent period physiology—the science which treats of the phenomena presented by living animals—had been for some time past but little cultivated in this country, but there has been of late years a great movement in advance. This movement is not special to physiology, but embraces physics, chemistry, pathology, and all the other branches of physical science. Laboratories for research and instruction in the sciences of light, heat and electricity, and of chemistry, no less than in physiology and comparative anatomy, have recently been established in Oxford and Cambridge. Physiological and pathological laboratories have been established in most of the large medical schools in the country. Much attention has been directed to the circumstance that, in the contract recently entered into for an addition to the University buildings at Edinburgh, provision is made for extensive appliances with a view to physiological experiment and instruction; while we are informed that until of recent years there was in this country no physical or physiological laboratory of any kind to which students had access, and in which they were regularly trained. There is, then, at the present time a general development of all the physical sciences,—what has been termed by one of the witnesses a great scientific revival. It is accompanied by the conviction, at which the men of science have generally arrived, that no teaching of physical science is complete unless it is illustrated by practical instruction. Physiology in particular is now for the first time assuming the position of a separate science. The professed physiologist has been until recently almost entirely unrepresented; that is to say, the physiological work has been done by persons engaged in other pursuits at the same time. The Professor of Physiology in Edinburgh was until a few years ago, a medical practitioner, a consulting physician; but the position is now filled by a professed physiologist, who occupies his whole time in that way. At present there are three positions in England, the tenure of which limits the holders to be exclusively professed physiologists, viz., Dr. Burdon-Sanderson's at University-College (London), Dr. Foster's at Cambridge, and the Brackenbury professorship at Owens College, Manchester. It is indeed the expectation of those most conversant with the subject, that physiological investigations will more and more take place in connection with public institutions, that new chairs will from time to time be founded, and that an organised system of instruction in physiology will speedily become an important feature in scientific education.

It is evident, therefore, that the number of experiments at present performed upon living animals can by no means be regarded as the limit of the number which we are called upon to include in our consideration, but that on the contrary we must assume that the experimental method is being rapidly developed,—very rapidly, Dr. Sharpey has assured us,—for he says, that the application of physics to the phenomena of life, particularly in making exact quantitative determinations, is one of the great characteristics of modern physiology.

In laying before Your Majesty our opinion as to the extent to which the practice now prevails, we have not the means of referring to statistical returns except as regards the experiments performed in the physiological laboratories attached to medical schools and universities; and there can be little doubt that experiments have been and now are performed occasionally by private persons, of whose number we are able to form no accurate computation. The number of persons systematically engaged in the performance of them does not appear to be more than from fifteen to twenty at the utmost. Such statistical information as we have been able to obtain will be found in the Appendix.

A very strong feeling has been excited in the country, within the last two or three years, on this subject.

On the occasion of the assemblage in London in 1874 of delegates from foreign countries connected with associations for the prevention of cruelty to animals, Your Majesty was graciously pleased to give public expression to a warm interest in the success of their efforts,—to a horror in hearing and reading of the sufferings which the brute creation often undergo,—to a fear that this is sometimes the case from experiments in the pursuit of science,—and to a hope that the entire advantage of those anæsthetic discoveries, from which man has derived so much benefit himself in the alleviation of suffering, may be fully extended to the lower animals.

We believe that these are the sentiments of Your Majesty's subjects generally. The present feeling appears to have been excited by a variety of concurrent circumstances, such as the movement, of which we have already spoken, involving, it is generally believed, a great increase, present and prospective, in the practice of subjecting live animals to experiments;—the introduction at some of the principal medical schools of experiments, by way, not of original research only, but of demonstrations to students given in public;—and the circulation here of the reports of many very painful experiments, mainly taken from foreign publications;—but most of all by the appearance in 1873 of a work called a Handbook of the Physiological Laboratory, professing to be intended for beginners, and describing many very severe experiments. It is admitted in his evidence by the editor himself, that no adequate means had been taken either to explain the meaning which was intended to be conveyed by the word “beginners,” viz., beginners in the special study of physiology, or to make known

what he told us is the general understanding in all English laboratories, that anæsthetics should be administered in the great majority of cases, and in other cases painful experiments should not be repeated merely to demonstrate truths already sufficiently established. This, it was presumed, would be taken for granted. He expressed his regret that this feeling should have been occasioned by the publication of the book, and gave us reason to expect that he would take measures, to which he referred, for correcting the impression it had produced. Much attention also has been drawn to a series of experiments which were recently performed by Dr. Ferrier in the laboratory of Dr. Crichton Browne, at the West Riding Lunatic Asylum at Wakefield. These have been widely made known to the public, and it has been asserted that from their nature they could not have been performed under anæsthetics. If that had been the case they would doubtless have been extremely painful, but they were performed in the presence of many medical men, and we are positively assured by Dr. Ferrier and Dr. Crichton Browne that all the animals experimented upon were first rendered insensible to pain, and were kept so during the operations. Much allowance must be made for the misunderstanding and exaggeration to which reports on such a subject are necessarily liable when not critically examined and not based upon the evidence of eye witnesses.

Medicine rests upon the triple basis of clinical observation, and pathological and physiological research,—and the practice of operating upon the living subject appears to have been coeval with the commencement of the pursuit of medical science, and to have been applied not only to animals, but at some times even to men, without regard to the suffering it involved. Speaking of the time of Celsus, Bacon, in the *Advancement of Learning*, says :—"As for the passages "and pores, it is true what was anciently noted, that the more subtle "of them appear not in anatomy, because they are shut and latent "in dead bodies, though they be open and manifest in live ; which "being supposed, though the inhumanity of anatomy vivorum was "by Celsus justly reprov'd, yet in regard of the great use of this "observation, the inquiry needed not by him so slightly to have "been relinquished altogether, or referred to the casual practices of "surgery, but might have well diverted upon the dissection of beasts "alive, which, notwithstanding the dissimilitude of their parts, may "sufficiently satisfy this inquiry."—Book II. Title X. 9.

Galen, in his writings, describes with painful particularity the great number of the tables he had, on which animals might be fastened, and the severe experiments which he used to perform upon them.

After the revival of letters, the practice was extensively pursued in the 16th and 17th centuries. Harvey appears to have been almost entirely indebted to it for the ever memorable discovery of the circulation of the blood. "When," he says, "I first gave my mind "to vivisections, as a means of discovering the motions and uses of

“the heart, and sought to discover these from actual inspection, and “not from the writings of others, I found the task so truly arduous “so full of difficulty, that I was almost tempted to think with “Fracastorius, that the motion of the heart was only to be compre- “hended by God. For I could neither rightly perceive at first when “the systole and when the diastole took place, nor when and where “dilatation and contraction occurred, by reason of the rapidity of the “motion, which in many animals is accomplished in the twinkling of “an eye, coming and going like a flash of lightning, so that the “systole presented itself to me, now from this point, now from that, “the diastole the same; and then everything was reversed, the “motion occurring, as it seemed, variously and confusedly together. “My mind was therefore greatly unsettled, nor did I know what I “should myself conclude, nor what believe from others. I was not “surprised that Andreas Laurentius should have said that the motion “of the heart was as perplexing as the flux and reflux of Euripus had “appeared to Aristotle. At length, and by using greater and daily “diligence, having frequent recourse to vivisections, employing a “variety of animals for the purpose, and collecting numerous ob- “servations, I thought that I had attained to the truth.” The deer in the Royal Parks were placed at his disposal for this purpose; and when he had arrived at such a point that he felt he could demonstrate the thing, King Charles the First and his Queen, with the princesses, attended a lecture of Harvey’s to witness the circulation of the blood proved upon the living body.

The great physiologist of the eighteenth century was Haller. Dr. Sharpey, after speaking of Haller as one of the greatest of physiologists and the author of the greatest work upon physiology perhaps that ever appeared, quoted to us the following extract from his writings, published in the middle of the century. “But it is not “sufficient to make the dissections of the dead bodies of animals. “It is necessary to incise them in a living state. There is no action “in the dead body; all movement must be studied in the living “animal, and the whole of physiology turns on the motions, external “and internal, of the living body. Hence no progress can be made “in investigating the circulation of the blood and its more recondite “movements, or the respiration, or the growth of the body and the “bones, the course of the chyle, or the motion of the intestines, “without the sacrifice of living animals. A single experiment will “sometimes refute the laborious speculation of years. *Hæc crudelitas “ad veram physiologiam plus contulit, quam omnes fere aliæ artes “quarum conspirante opera nostra scientia convaluit.*” But it must not be supposed from this passage that Haller was an advocate of cruelty; for in his *Life* by Dr. Henry of Manchester it is recorded that he was convinced that an idle inquisitiveness or a passion for reputation could not justify our killing sensible beings in torments, and that whatever reason we may have to regard them as formed for our use,

it is absurd and cruel to imagine that they are designed also to be the sport of our curiosity or vanity.

We have great satisfaction in assuring Your Majesty that at the present time a general sentiment of humanity on this subject appears to pervade all classes in this country. "It deserves detestation and abhorrence," is the emphatic reply of Mr. Darwin, when the question is put to him what would be his view of trying a painful experiment without anæsthetics when the same experiment could be made with anæsthetics,—or in short of inflicting any pain that was not absolutely necessary upon any animal. This principle is accepted generally by the very highly educated men whose lives are devoted either to scientific investigation and education, or to the mitigation or the removal of the sufferings of their fellow creatures; though differences of degree in regard to its practical application will be easily discernable by those who study the evidence as it has been laid before us. Eminent as Mr. Darwin is in science, he has been but little conversant with experiments on living animals. Of those who have practised such experiments from their earliest years of study, none is more distinguished than Dr. Sharpy. He assures the Commission that when he was a very young man studying in Paris, he went to the first of a series of lectures which Majendie gave upon experimental physiology, and was so utterly repelled by what he witnessed, that he never went back again. This was no small evidence of humane sentiment on the part of a young and ardent student at the outset of his professional career, for it involved the sacrifice of what was probably the principal object of his residence in Paris; and he does not hesitate now, in his riper years, to speak of one of Majendie's experiments in particular as "his famous, it might rather have been called infamous" experiment.

The feeling of the students on the subject is thus described by their teachers. Dr. Pavy says that at the commencement of his lectures he is almost obliged to give a little apology for saying that the course will be an experimental one. He sees upon the faces of the students sitting before him an expression which leads him to consider it necessary to make some explanation, and to tell them at once that no experiment will be introduced which will wound the feelings of the most sensitive amongst them. It is what he is constantly in the habit of doing, and he has found it necessary from what he has observed in his audience. He is speaking of his own class at Guy's Hospital, but this he says we may take as a fair average example of the students at all well conducted schools of medicine in the country. Dr. Rutherford told us, speaking of the students at Edinburgh, that if an animal has been suffered to come out from anæsthesia, the students at once resent it. Dr. McDonnell, speaking of the students in Dublin, says that unless he was able to give some good reason for doing away with the anæsthetics, the students would not tolerate the occurrence; the public opinion of the students would be strongly

against it. Sir William Gull assures us that in a medical school anything like cruelty or indifference to suffering would be scouted by the public opinion of the students, and that he has never seen anything like indifference.

Dr. Houghton told us that he was present in Norwich on the occasion when the experiments upon dogs were performed which led to the celebrated prosecution, and the public was so much shocked that he thinks M. Magnan, who performed them, was in danger. Of this experiment we are told by Professor Humphry, who was present during a portion of the time, that it was an experiment of some scientific value, and the dog he saw did not appear to be suffering much. Sir William Fergusson told us he thought it was grounded on incorrect views altogether, and Dr. Taylor that the experiments were of a most cruel kind and could answer no sufficient purpose.

The Secretary of the Royal Society for the Prevention of Cruelty to Animals, when asked whether the general tendency of the scientific world in this country is at variance with humanity, says he believes it to be very different indeed from that of foreign physiologists; and while giving it as the opinion of the society that experiments are performed which are in their nature beyond any legitimate province of science, and that the pain which they inflict is pain which it is not justifiable to inflict even for the scientific object in view, he readily acknowledges that he does not know a single case of wanton cruelty, and that in general the English physiologists have used anæsthetics where they think they can do so with safety to the experiment.

The recommendations which we shall humbly submit to Your Majesty will turn in a great measure upon the use of anæsthetics. The whole subject of experiments upon living animals has been, or at least ought to have been, relieved of the greater part of its difficulty by the discovery of anæsthetics and particularly of the anæsthetic properties of the vapours of sulphuric ether and of chloroform, in 1846 and 1847.

In considering what those recommendations ought to be, the first question that offers itself is. Can the practice of subjecting living animals to experiments be altogether prohibited.

To this question it may seem sufficient to reply that such an enactment must inevitably lead either to a general evasion of the law, or to an universal flight of medical and physiological investigators and students from the United Kingdom to foreign schools and laboratories, and that by this means the general treatment of animals in experiments would certainly not be altered for the better. We have not been entitled to expect the attendance of foreign witnesses; and it is obvious that our recommendations, if approved and adopted by Your Majesty, can have no force in foreign countries. We have, therefore, thought ourselves bound in fairness to avoid receiving adverse testimony in respect to foreign physiologists beyond what is to be derived from their own published writings; and it has given us

sincere pleasure to receive incidentally from competent witnesses an assurance of the humane spirit which they have known to prevail in some foreign laboratories. We are, nevertheless, fully justified in asserting that nothing will be gained to the cause of humanity by compelling native students to seek their education abroad instead of at home ; and it is evident that the voice of public opinion here will have an influence in the great public institutions of this country, which it cannot exercise if experiments on living animals shall be driven into private chambers to be carried on, with inferior appliances, in conscious violation of the law.

But even if it were possible to accomplish the purpose by legislative prohibition, would it be reasonable to do so ?

No one is better entitled to speak with authority on this subject than Sir Thomas Watson, not only on account of the high esteem in which he is held on professional and personal grounds, but also because the objection which prevails in many minds against all who have been accustomed to witness the infliction of pain is totally inapplicable to him. His practice, it is well known, has been that of a physician, and we learn that he has attained to his eminence in the profession without having ever himself seen an experiment on an animal at all,—making himself acquainted with those performed by others and their results, and making such use of them as he could. When the present controversy began, Sir Thomas Watson published, in a periodical of the day, his opinions upon it. This paper will be found in our Appendix ; and his views will appear, in the minutes of our evidence, to have been sustained by himself and supported by the general concurrence of the highest medical and scientific testimony. He begins with the general proposition that, at a small expense of suffering to one of the lower animals, we may obtain knowledge which enables us to prevent or mitigate pain much more severe and lasting, or even to ward off peril to life, or to prolong life in a human being. He states his opinion that for such purposes experiments are justifiable ; but that they ought to be made under very great restraint, and with very careful forethought ; that no such experiments are excusable if they are made at random ; that to justify them at all there must be some definite object in view of a previously instructed mind ; some plain question to settle, some important doubt or uncertainty to remove, some hypothesis containing the promise of service to humanity to be confirmed or confuted, at least some reasonable hope and prospect of resulting benefit ; that the experimenter ought to be an absolutely good anatomist, that he ought also to be master of all that had hitherto been learnt respecting the question which he was endeavouring to elucidate, and that he should take especial care to have the proper implements and apparatus at hand for the performance of his experiments, and that he should have trained assistants who would not blunder in assisting him. Sir Thomas Watson says that, since the introduction of

anæsthetics, the great majority of experiments can be rendered very nearly, if not entirely, painless. He further states that it is the decided wish of the most eminent men in the medical profession in both its branches to prevent abuse and to repress cruelty as much as possible, and he expresses his confident opinion that the Crown and Parliament may look for the support of those eminent men in any reasonable measures which they may adopt for that purpose.

In a kindred spirit, Sir James Paget, who is President of the Royal College of Surgeons and of the Medical and Chirurgical Society, laid before us a series of resolutions which were passed at the meeting of the British Association in Edinburgh in 1871. Those resolutions were:—“(I.) No experiment which can be “performed under the influence of an anæsthetic ought to be done “without it. (II.) No painful experiment is justifiable for the mere “purpose of illustrating a law or fact already demonstrated; in other “words, experimentation without the employment of anæsthetics is “not a fitting exhibition for teaching purposes. (III.) Whenever, “for the investigation of new truth, it is necessary to make a painful “experiment, every effort should be made to ensure success, in order “that the sufferings inflicted may not be wasted. For this reason, “no painful experiment ought to be performed by an unskilled “person, with insufficient instruments and assistants, or in places not “suitable to the purpose; that is to say, anywhere except in “physiological and pathological laboratories, under proper regulations. “(IV.) In the scientific preparation for veterinary practice, operations “ought not to be performed upon living animals for the mere “purpose of obtaining greater operative dexterity.”

Sir James Paget proceeded to say that these resolutions had received his entire approval. The principle of them was adopted in a petition signed by Mr. Darwin, Professor Owen, Mr. Huxley, Sir William Gull, Sir William Jenner, the President of the College of Physicians, and several more leaders in science:—and the Bill which was introduced into the House of Commons by Dr. Playfair in the last Session of Parliament must (without committing to all its provisions all who were favourable to it generally) be accepted as a proof of the readiness of men of the highest eminence in science to submit to the consideration of the legislature this difficult question.

The opinion that experiments upon animals susceptible of pain ought not to be altogether prohibited, even if it were possible to do so, has been supported before us by reasoning which it is not easy to confute. All medical science, it is urged by Sir James Paget, being in a state of progress, is continually coming in sight of problems which can only be solved by experiments either upon man or upon some lower animal; so that if a patient suffering from injury or disease is brought to a physician or surgeon, the physician or surgeon must either adopt some course of cure which has become known to him from the former experience of the profession acquired by

practice upon other patients or by experiment by himself or others on animals, or else he must adopt upon conjecture the remedy which he thinks most likely to be useful ; that is to say, in other words, he must submit the present patient to an experiment, instead of proceeding to his treatment with the assurance which might have been given by past experience. "We save ourselves," Mr. Simon expresses it, "a great deal of needless experimentation on man by some 'experimentations on the lower animals.'" This cogent argument is evidently susceptible of indefinite illustration from memorable cases which have from time to time occurred in history.

We propose to consider experiments upon animals under three heads,—operations involving the use of the knife,—the administration of poisonous or dangerous drugs,—and the production of disease; but before we do so it may be well to observe that in a large proportion of the experiments made for scientific purposes either no pain at all need be inflicted, as in the familiar instance of examining the circulation of the blood in the web of a frog's foot under the microscope ;—or the animal is first permanently deprived of sensation, as in the study of reflex movements of muscular irritability, of the action of the heart in cold-blooded animals, in the maintenance of the heart's action by artificial respiration, and other cases.

It may also be convenient to notice that two classes of experiment are usually spoken of in the medical profession under the common appellation of therapeutic experiments,—*i.e.*, experiments made with the view of ascertaining the properties of new remedies, or of acquiring greater precision in the knowledge of the action of those already in use. Therapeutic experiments are sometimes conducted by operations upon living animals, as when the organ, for example the liver, is exposed, in order that the effect of some agent upon that organ, whether a well-known agent or a newly discovered or suggested agent, may be open to view and be observed :—or they may consist in simply trying upon a living animal the effect of an agent which it is proposed to introduce, but which it is thought proper to try upon animals before the trial is hazarded upon man. In the tripartite division of experiments which follows, the former of these classes will be included under the head of operations, the latter under that of the-administration of drugs.

Experiments upon living animals then may be considered under three principal heads, viz. :—

a. Operations :—These are performed for the purpose of examining, either for original research or for demonstration to students, the processes of life.

b. The administration of poisonous or dangerous drugs :—for the purpose of exhibiting the effect, or of discovering the cure,—or for the purpose of assisting legal investigations.

c. The production of disease :—for the purpose of observing its progress, and discovering the means of preventing, mitigating, or

curing the effects of the same or similar diseases in men or animals.

a. The principal cases which have been selected for us as instances in which such operations have resulted in great discoveries have been the discovery of the circulation of the blood,—the discovery of the action of the lacteal and lymphatic system of vessels,—and Sir Charles Bell's discovery of the compound function of the spinal nerves; these three subjects have been selected for illustration because they lie at the very foundation of our present physiological knowledge. Professor Turner, of Edinburgh, to whom we have been indebted for this selection, has submitted to us notes of the history of various discoveries, and we desire to direct the particular attention of all who wish to examine the extent to which practical medicine has been improved by physiological experiment to his evidence and that of Dr. M'Kendrick on the subject.

It would require a voluminous treatise to exhibit in a consecutive statement the benefits that medicine and surgery have derived from these discoveries. Let us take for our example the discovery of the circulation of the blood, and the various improvements in the treatment of diseases, and in the safer method of performing surgical operations on the human subject, that have resulted from it. In medicine it is obvious that a knowledge of the nature of, and of the proper treatment to apply in the large and important class of diseases of the heart and blood-vessels, could not have been acquired without a knowledge of the nature of the mechanism of the circulation. In surgery, this discovery has exercised a still more direct influence; and the narrative of the improvements in practice directly referrible to it would lead us by gradual and successive stages from the time when after an amputation red-hot irons were applied to staunch the bleeding vessels, to the employment of the carbolized ligature of the present day. If Harvey's experiments, and those upon the lacteal system, were to be performed now, the animals would first be rendered insensible to pain; and even in the case of Sir Charles Bell's experiments, where sensation was the immediate subject of the investigation, by far the most severe part would also be performed while the animal was in a state of complete anæsthesia.

If, therefore, this inquiry had been instituted at some former time, we should have been compelled, in regard to this class of experiment, to weigh in the one scale the infliction of great and perhaps protracted suffering,—and in the other the sacrifice of knowledge, most important to mankind, but only to be looked for from experiments which involved such suffering. By the discovery of anæsthetics we have been relieved from that necessity, and our present task is to devise measures which may prove effectual to prevent abuse.

b. With reference to the administration of poisons to the lower animals, Sir James Paget brings forward the hope of discovering an antidote for snake poisons, in proof of the absolute necessity for the

performance of these experiments. He mentions the many thousands of Your Majesty's Indian subjects who perish annually from snake bites. It is hopeless, he says, to make observations upon the persons bitten by the snakes, with the expectation of effecting a cure. A Government reward has been offered for the discovery of a sufficient antidote. Sir James Paget mentions the case of an enthusiast who was so sanguine of obtaining this reward that he was with difficulty restrained from subjecting himself to the bite of the cobra in proof of the efficacy of his antidote. The medical authorities of St. Bartholomew's Hospital persuaded him to transfer the experiment to some pigeons, which all instantly died. Sir James Paget says we cannot expect that short of fifty years hence a true antidote for snake bites should be found by any other means than by a series of experiments.

Of the advantages derived to man from the administration of drugs to animals by way of experiment, Sir James Paget gives an illustration in the case of that fatal and distressing malady angina pectoris, which appears quite recently to have been brought in some degree within the domain of medical control, in consequence of a discovery made from observing the effect of nitrite of amyl on living animals. It must not be forgotten that for the discovery of anæsthetics themselves both men and animals have been largely indebted to the practice now in question.

On the subject of administering poisons to animals in furtherance of medico-legal jurisprudence we examined Dr. Taylor, lecturer on medical jurisprudence and toxicology at Guy's Hospital, the author of well-known works upon the subject. Dr. Taylor has been a principal witness on all great cases of poisoning for the last 20 years. He referred us especially to his experience in the case of Palmer, Smethurst, and Pritchard. He tells us that in the case of Palmer, the conviction resulted from evidence given by himself and Dr. Rees. The effect of strychnine was at that time little known. Great difficulty was occasioned to the prosecution by the mode in which the viscera were handled after they had been taken from the body. The main point in controversy was whether it was possible that the poison could have been the occasion of the death, seeing that it was not found in the contents of the stomach. This possibility was established by experiments upon six rabbits made by Dr. Taylor and Dr. Rees. Again, Dr. Taylor referred us to a case which occurred in Suffolk twelve or fourteen years ago, in which a woman was charged with having administered arsenic to her stepchild, who had clearly died from the effects of that poison. Dr. Taylor and Dr. Pavy established by experiments upon rabbits and dogs what satisfied the coroner's jury that the presence of arsenic in the stomach was not inconsistent with the statement that the death had been occasioned by the admitted application of arsenical ointment to the skin. These cases might be multiplied indefinitely.

It is not possible for us to recommend that the Indian Government should be prohibited from pursuing its endeavours to discover an antidote for snake bites:—or that without such an effort Your Majesty's Indian subjects should be left to perish in large numbers annually from the effects of these poisons; nor can we say that new medicinal agents ought in the first instance to be tried upon man, when the risk to human life might have been prevented by a previous trial upon animals. We cannot recommend that for want of such experiments criminals like Palmer should be permitted to escape, or persons suspected be deprived of the means of establishing their innocence. But we think it is most desirable that an effectual restraint should be placed upon what Dr. Taylor has described to us as purposeless cruelty,—on experiments made in excessive numbers,—on experiments made to establish what has been already proved,—on experiments attended with great pain, and defeating the very object in view, on experiments made where a man has been desirous of bringing himself forward, or trying a new thing merely for the sake of a little notoriety.

c. The production of disease in animals has been already, and is likely to become still more, the source of knowledge prophylactic as well as therapeutic, tending, in the most important degree to the prevention as well as to the mitigation and the cure of disease in the human family. There can be no simpler, or more striking example than that of vaccination for the small-pox, the discovery which was said to have saved more lives than all the wars of Napoleon had destroyed. It was by observation that Dr. Jenner discovered the immunity from small-pox of those who had contracted cow-pox. But it was by experiments upon cows that the origin of the cow-pox, a disease stated to be derived from grease in the horse, was ascertained. At the present time systematic experiments are being made under the direction of the Committee of Your Majesty's Privy Council for the purpose of establishing facts which may guide that department in taking efficient measures for the prevention or for the cure of disease. In an explanatory memorandum given to Mr. Forster when Vice-President by Mr. Simon, the medical officer, it is stated that the first aim is to obtain exact scientific knowledge of the causes and mode of attack of any disease which is in question; and in this study it frequently happens that more or less experiment has to be made as to the results which the administration of a particular influence will produce on an animal. Studies of this kind are sometimes made more immediately in the interests of man, as for instance in the case of the Asiatic cholera, and sometimes in that of the domestic animals, as in the case of sheep-pox and cattle plague, but perhaps oftenest in the common interests of both. Mr. Simon states in his memorandum that being obliged, as medical adviser of the Government, to make himself as proficient as he can in all that relates to the preventing and resisting of disease, he has felt it indispensable to

have recourse *inter alia* to such studies. The subjects to which his investigations have been particularly directed have been cholera, tubercle, pyæmia, sheep-pox, and disinfectants. Much of the practical advice given by the Department to the public as to how they shall deal with the cases of cholera which may arise, and the precautions they shall take against the spreading of the disease, is founded on the basis of such experiments. The mode in which cholera is spread had indeed been suspected. It was a theory in the teaching of Dr. Snow in 1849, but Mr. Simon states that since that time it has been converted by experiment into a certainty which can be made a basis for the advice of a Government department. Important knowledge has also been acquired in respect of tuberculosis, that fatal malady which causes the loss of one tenth of the whole number of the human family who die in the United Kingdom. This knowledge may be expected to receive fresh development, and to lead, if not to the suggestion of any cure, at least to the avoidance of many of the causes which now occasion the production of the malady in the human subjects.

For other instances of a like nature, we must refer to the evidence. The deduction we draw from them is that, whether we look to the possibility of cure or to the probability of prevention, we cannot recommend the total prohibition of experiments of this class. It consists in subjecting a comparatively very small number of animals to diseases not generally involving severe pain,—and from the observation of these diseases results are likely to be derived tending to the mitigation, or possibly even the removal, of some of the severest scourges which afflict the human race.

But if it be impossible altogether to prevent experiments, and would not be reasonable to do so,—even if it were possible,—under what aspect ought they to be regarded by the law?

The law at present takes no special cognizance of the subject, but leaves it to the operation of the statute 12 and 13 Vict. c. 92, commonly known as Martin's Act, if indeed that Act should be held to be applicable; and the Act extends only to domestic animals, leaving frogs, rabbits, guinea pigs, and other animals entirely unprotected. It gives, that is, in respect of domestic animals, a remedy doubtful, even if applicable, and not easily enforced, and in respect of others no remedy at all.

In considering the question of legislative interference, we have found in some minds a decided prepossession against it. This appears to be connected, as in the case of Mr. Lister, with a notion that such interference implies an imputation of cruelty upon those who are engaged in these investigations; an imputation they are conscious they have not deserved. From this prepossession, as we have already seen, many of those whose position and character entitle them to the greatest weight are wholly free; and it has almost always yielded to the consideration that if there be a proved necessity for

legislative interference to prevent abuse, such interference will be right, provided that the teaching of physiology and the prosecution of research by competent persons are not interfered with. Sir Thomas Watson, Sir George Burrows, Sir James Paget, and many others have suggested the analogy of the Anatomy Act, which has worked very satisfactorily for its purpose, and some scientific witnesses have expressed their opinion that the interference of the legislature is called for in the interest not only of humanity, but also of science. Sir James Paget considers that the present practice of the medical schools may be an error; no such experiments should be done, he says, except with the consent of a Committee. He thinks it unreasonable to suppose that four members of the medical profession would join in approving an unnecessary or unreasonable experiment,—though one in his zeal might do so, four certainly would not.

Sir William Fergusson thinks that experiments which involve suffering are carried to a greater extent than they need be, and that there is continued and useless repetition. His own opinion is much less favourable to these experiments than it was when he was young, because he had much less grasp of the subject at that time. The more matured judgment of his later years has led him to say to himself that he would not perform some of the operations now that he performed in his earlier years. He thinks if the public really knew what was actually going on in this country at this time, they would expect an interference on the part of the Crown and Parliament just as much as with reference to the disinterring of dead bodies years ago. He laid before us a protest,—on the subject, we have been told, of cruelties alleged to have been practised at the Veterinary College at Alfort,—signed in 1867 by the late Mr. Syme, and other eminent surgeons, as well as Mr. Wilkinson, the principal veterinary surgeon to the forces.

Of those who are most directly occupied in purely physiological instruction and research,—Dr. Burdon-Sanderson says that the state of things which he would like to see established with reference to physiological research, is such as would unquestionably discourage the making of experiments by any one, excepting by persons trained in a school of physiology. He thinks there would be some inconveniences attaching to legislation, but also that there would be even for physiology some advantages. The difficulties would apply with reference to private individuals, but though he thinks it would be an objection if private individuals should be interfered with, he does not lay great stress upon that, because they are few and will probably become fewer year by year. As research is carried on into the more difficult parts of physiology, the investigator requires appliances of greater complexity, which are exceedingly expensive, and even if he could afford to buy them, he would have to build a place adapted for their use. Dr. Burdon-Sanderson was an assenting party to Dr.

Playfair's Bill, having taken an active part in regard to it, in the main approving of it, and thinking it a suitable measure.

Dr. Ferrier, Professor of Forensic Medicine at King's College, London, thinks that any legislation that would retard physiological research would be a discredit to the country; but that owing to the great agitation which has taken place, it is very desirable to dissociate experiments for the purpose of original research from cruelty to animals, or the law which punishes that cruelty, and that some legislation for that purpose would be desirable.

Professor Rolleston of Oxford thinks we may bring the forces of society to bear upon the individuals. His impression of the English nature is that it is a law abiding nature, and that a pronunciamiento on the side of carefulness would have its effect. He is himself amenable to the visits of the anatomy inspector, and does not feel in the least annoyed by them. He is of opinion that any experiments worth doing will be done in a public laboratory, and that no great inconvenience would arise from compelling all experiments to take place in a laboratory amenable to the visits of an inspector.

Dr. Michael Foster, the Prælector of Physiology in Trinity College, Cambridge, is not prepared to go quite so far as Dr. Sanderson in the matter of the necessity for legislation, inasmuch as so far as his experience has gone in this country, he thinks there have been no abuses, and the humane sentiment which prevails might of itself be trusted entirely to prevent such abuses; but he would not object to measures which might prevent physiological experiments falling into the hands of other people of less humane sentiments if it should appear to be necessary. He does not object to legislative interference; but he does not so far agree with Dr. Sanderson as to think it desirable or necessary.

Dr. Gamgee, Brackenbury Professor of Physiology in Owens College, Manchester, thinks that licenses might be given with great advantage if no vexatious spirit guided those who gave them. Having in view a considerable increase of great schools, he sees no objection to the establishment of some reasonable regulations for such institutions; and has no sentiment of an affront offered to himself in the proposal of regulations which, while they did not limit the progress of science, and the efforts of competent scientific people, should restrain the performance of operations upon living animals by incompetent persons, for no definite object, and without proper precautions.

Dr. Rutherford, Professor of the Institutes of Medicine and Physiology in the University of Edinburgh, says, that it might be almost a dangerous thing for the power of granting a license to be vested in the Secretary of State: and it would be better to vest it in the councils of the Royal Society and other scientific societies, under whose jurisdiction the Inspector of Anatomy might act; and these councils might in turn be responsible to the Secretary of State.

Dr. Playfair's bill, he thinks, was objectionable because it would have prohibited an experiment for the purpose of demonstration even under complete anæsthesia; and there are some minor ones which cannot well be performed under anæsthesia, such as demonstrating the action of strychnia. With these exceptions, neither Lord Henniker's bill nor Dr. Playfair's would interfere with the proceedings of a competent and well organized school like that of the University of Edinburgh; but would have the effect of interfering with persons of a totally different description, if there should be any such, who employed themselves in such pursuits. In organized schools there are securities against abuse which do not exist elsewhere.

Dr. Handyside, Teacher of Anatomy in the Edinburgh School of Medicine, suggests that a system of licensing would afford the general public ample security that the practice is nowhere being carried on to any objectionable extent. He refers to the analogy of the Anatomy Act, as supporting a system of inspection. He thinks that the control should in some degree be entrusted to laymen, and proposes that justices of the peace should certify to the fitness of the persons applying for licenses, to prevent the profession being without a check.

Dr. Samuel Haughton, the Medical Registrar of the School of Physic of Trinity College, Dublin, the largest school in Ireland, tells us that vivisections are strictly prohibited, and that the opinion of the educated public in Ireland is very sensitive on the subject of vivisection for the purpose of teaching classes. As regards original research, he thinks there is a good deal of second-rate physiological practice going on, which needs control; that the conscientiousness and common sense and savoir faire of young physiologists ought not to be trusted without it. His opinion is that the practice should not be allowed at all for teaching purposes; that for purposes of original research it ought to be tolerated, but regulated:—the supervision having an analogy to, or being based upon, the provisions of the Anatomy Act.

For the opinions given by other witnesses we refer to the minutes of evidence.

But even if the weight of authority on the side of legislative interference had been less considerable, we should have thought ourselves called upon to recommend it by the reason of the thing. It is manifest that the practice is from its very nature liable to great abuse; and that since it is impossible for society to entertain the idea of putting an end to it, it ought to be subjected to due regulation and control. Those who are least favourable to interference assume, as we have seen, that interference would be directed against the skilful, the humane, and the experienced. But it is not for them that law is made, but for persons of the opposite character. It is not to be doubted that inhumanity may be found in persons of very high position as physiologists. We have seen that it was so in Majendie. Dr. Carpenter told us that he has seen in many instances

a perfect callousness to animal suffering before the introduction of anæsthetics, a callousness which strongly repelled him. That very severe experiments are constantly performed cannot be doubted by those who read the documentary evidence which has been laid before us, and the testimony of Dr. Walker and other witnesses who speak from personal knowledge of the sufferings which they say have been often unnecessarily inflicted in the name of science. Dr. Anthony, who resides in the neighbourhood of Birmingham, told us that he is acquainted with instances of young men who carry on experiments in private houses from mere curiosity. He considered that he had obtained his knowledge confidentially, and declined to give us any further clue to the individuals than to say that he has a consciousness that the thing has been done, is done, and probably will be done. We have had some evidence that cases have arisen in which the unpractised student has taken upon himself, without guidance, in his private lodgings, to expose animals to torture without anæsthetics for no purpose which could merit the name of legitimate scientific research. Evidence of this nature is not easily obtained. So far as our evidence goes, such cases appear to have been exceptional and abnormal, but the repetition of them is not on that account unworthy to be guarded against by a legislature desirous of giving effect to the moral sense of the community at large. Besides the cases in which inhumanity exists, we are satisfied that there are others in which carelessness and indifference prevail to an extent sufficient to form a ground for legislative interference. We have not thought it part of our duty, the majority of us not having had professional training, to decide upon matters of differing professional opinion, but we have been much struck by the consideration that severe experiments have been engaged in for the purpose of establishing results which have been considered inadequate to justify that severity by persons of very competent authority. Cases may not improbably arise in future in which the physiologist may be disposed to underrate the pain inflicted in the course of establishing results which may prove to be trivial, or even worthless. Looking to the circumstance that a great increase is to be expected in physiological inquiry, it appears to us most important that some legislative control should be established to prevent abuse extending in this direction. It is, moreover, much to be regretted that a feeling of suspicion, and even of abhorrence, should have been permitted to grow up among a large and very estimable portion of the public, against those who are devoted to the improvement of medicine and to the advancement of science. Publicity is the antidote of suspicion, and we look to the reasonable superintendence of constituted authority as affording the means of reconciling in the public mind the sentiment of humanity with the desire for scientific knowledge.

Our conclusion, therefore, is that it is impossible altogether to prevent the practice of making experiments upon living animals for

the attainment of knowledge applicable to the mitigation of human suffering or the prolongation of human life :—that the attempt to do so could only be followed by the evasion of the law or the flight of medical and physiological students from the United Kingdom to foreign schools and laboratories, and would, therefore, certainly result in no change favourable to the animals :—that absolute prevention, even if it were possible, would not be reasonable :—that the greatest mitigations of human suffering have been in part derived from such experiments :—that by the use of anesthetics in humane and skilful hands the pain which would otherwise be inflicted may, in the great majority of cases, be altogether prevented, and in the remaining cases greatly mitigated :—that the infliction of severe and protracted agony is in any case to be avoided :—that the abuse of the practice by inhuman or unskilful persons,—in short the infliction upon animals of any unnecessary pain,—is justly abhorrent to the moral sense of Your Majesty's subjects generally, not least so of the most distinguished physiologists and the most eminent surgeons and physicians :—and that the support of these eminent persons, as well as of the general public, may be confidently expected for any reasonable measures intended to prevent abuse.

With these views we have examined the measures which were introduced into the two Houses of Parliament in the last session, as well as the bill which has been submitted for our consideration by the Royal Society for the Prevention of Cruelty to Animals, and we now propose humbly to recommend to Your Majesty the outline of a measure which, in the interest both of humanity and of science, we think it will be expedient to adopt. But before doing so we will briefly state our views upon certain points of interest which have come before us in the course of our inquiry.

It has been proposed to enact that the object in view shall be some immediate application of an expected discovery to some prophylactic or therapeutic end, and that any experiment made for the mere advancement of science shall be rendered unlawful. But this proposal cannot be sustained by reflection upon the actual course of human affairs. Knowledge goes before the application of knowledge, and the application of a discovery is seldom foreseen when the discovery is made. The first origin of a great discovery is often like the germ of the natural life in an animal or a vegetable, so small as to be scarcely perceptible, and yet it may contain in it the seeds of the grandest results. "Who," says Helmholtz, "when Galvani touched the muscles of a frog with different metals, and noticed their contraction, could have dreamt that all Europe " would be traversed with wires, flashing intelligence from Madrid to " St. Petersburg with the speed of lightning? In the hands of " Galvani, and at first even in Volta's, electrical currents were phæno- " mena capable of exerting only the feeblest forces, and could not " be detected except by the most delicate apparatus. Had they

“ been neglected, on the ground that the investigation of them promised no immediate practical result, we should now be ignorant of the most important and most interesting of the links between the various forces of nature. Whoever, in pursuit of science, seeks after immediate practical utility, may generally rest assured that he will seek in vain.” As we have seen, Harvey’s great discovery has been followed by remedial consequences of inestimable value, but those consequences were not foreseen by himself at the time that he made the discovery. It was, at the time that he made it, “ a mere scientific discovery.”

Again, it has been proposed to sanction experiment for original research, but to prohibit it as far as regards demonstrations to pupils. In the regulations issued by the Royal College of Surgeons, attendance upon physiological lectures is required from candidates for the medical profession ; but it is stated that it is not expected that learners shall perform vivisections. We are told by Dr. Acland, Regius Professor of Medicine in the University of Oxford, the President of the Medical Council, that the question of the extent to which practical physiology is necessary for students would have been brought before the Medical Council this year, at the session which has lately closed, had it not been for the appointment of our Commission,—and that at his suggestion it was delayed until after we should have made our report to Your Majesty. It seems to us that living animals ought not to be subjected to experiment at all for any purpose of ordinary education. But in the case of professional education, as at one of the medical schools, it cannot, we think, be denied that there is much force in the argument that teaching without demonstration can scarcely be considered teaching. With respect to the medical schools we accept the resolution of the British Association in 1871, that experimentation without the use of anæsthetics is not a fitting exhibition for teaching purposes ; and whatever may be the hazard of evasion in private chambers, we think there can be none in the public lecture room of a great institution, when Parliament shall have established the obligation and the Crown have undertaken to enforce it.

A collateral argument has been brought forward in favour of this distinction, aiming not at the saving of pain to the animal, but at the saving of demoralization to the student. But the tendency to demoralization is connected, as the shadow with the substance, with the rightness or wrongness of the thing itself ; and the evidence we have quoted above seems to show conclusively that at the medical schools where such demonstrations are exhibited under anæsthetics the sense of humanity in the students is not in fact impaired.

It has been proposed to prohibit experiment except in public halls, to which a certain portion of the non-professional public shall at all times have access. It appears to us that this proposal would, if carried into effect, tend to frustrate the experiment as regards its usefulness, and, perhaps, as regards the effective administration of

anæsthetics also, since the most essential requisite for the conduct of a delicate experiment is that the person who makes it should be free from any mental interruption or disturbance; while the presence of ignorant spectators could do nothing to secure the real humanity of the experiment. An animal may be suffering exquisite torture, and yet (so far as we yet know) the worari poison may, by its effect upon the motor nerves, prevent the exhibition of any feeling. Or on the other hand, an animal may make every demonstration of suffering while the real sensation is destroyed. In the human subject, when chloroform is employed, or when by an injury to the spine the connection with the brain is interrupted, it sometimes happens that all the outward manifestation of pain is exhibited, when the patient afterwards disclaims having experienced any sensation of it. These effects are perfectly familiar to the instructed, but would be simply misleading to the uninitiated.

A good deal of evidence has been given as to the effect of the poison called worari or curari. This poison is very convenient to an operator, since it paralyses the motor nerves and keeps the animal quiet. It has however been positively stated by perhaps the highest authority on such a subject, Claude Bernard, to have no effect in producing insensibility to pain. This opinion is now beginning to be disputed, but we think that until the question shall be much better settled than it is at present this poison ought not to be regarded as an anæsthetic by those who administer the law in respect of experiments on animals.

Some physiologists, while quite ready to assert broadly the principle that so far as is possible no painful operation ought to be performed except under anæsthetics, do not extend the application of the principle to cold-blooded animals. Of this kind of animal the typical instance is the frog, in which there has sprung up an import trade for the purpose of experiment, the large frog of Germany not being indigenous here. Dr. Schafer, the assistant professor of physiology in University College, after saying that the treatment of animals there is, he is quite certain, dictated most strictly by a sentiment of humanity, nevertheless goes on to say that they consume a large number of frogs, and that the experiments upon living frogs are usually performed without anæsthetics, because the opinion is that the frog is not so sensitive as the higher animals, and he says that no special precaution is taken to diminish pain in their case. This doctrine is one which ought not to be too readily admitted, for the question is an important one; the proportion of frogs to all the other animals put together that are subjected to experiment is so great, that the frog, we are told, is called the physiologists' animal. Dr. Gamgee tells us on the contrary, that when he decapitates a frog he always crushes the brain, and explains to the students why he thinks that the brain might otherwise retain a sensibility, which would not be retained in a warm-blooded animal.

It is said by some of the witnesses that it is very difficult to subject frogs to chloroform. But this is contradicted by the experience of others whose authority is indisputable, as for example Professor Humphry and Dr. Brunton; and Dr. M'Donnell tells us that he constantly uses it for frogs, and even for tadpoles, and that if a little chloroform is diffused through water, and the tadpole is put swimming about it for a few minutes, there is enough absorption through the skin to render it soon insensible, and it can be placed under the microscope for half an hour or so, and not stir. We think that in framing rules for the administration of a system, there ought to be much jealousy in too readily admitting convenient doctrines, and that proper care should be taken to insist upon the removal of the sensibility to pain, even in the case of cold-blooded animals.

Mr. Pritchard, Professor of Anatomy in the Royal Veterinary College, performs most of the operations there. He says that they have no operations for the purpose of experiments, and thinks it would be improper that they should. He is not aware of any being performed in this country by veterinary surgeons, does not think it is the practice, and, if it were, the principal of the college, Professor Simonds, and persons in positions like his own, would be ready to support the Government in any reasonable measures of regulation. In the New Veterinary College at Edinburgh the operations are, as a rule, performed by the principal, Mr. Williams. He admits as respects one, but only one, important operation, that it is occasionally performed for teaching purposes, but he says that the animals are always rendered insensible by chloroform. The principles of our report apply to the practice of veterinary surgeons, and they, like the rest of the community, are included in the purview of the measure we are now about to recommend.

We have spoken of three bills which have been submitted to us. They will be found in the Appendix. All these bills proceed upon the basis of license by the Secretary of State, subject to withdrawal in case of abuse,—and of adequate inspection. This basis, as we have already seen, was laid down also in the evidence of Sir Thomas Watson, and of other most competent witnesses, and upon it we think an effectual measure for the prevention of abuses may be founded.

We were reminded by the secretary that the Royal Society for the Prevention of Cruelty to Animals is not the society established for the total abolition of experiments. The Royal Society for the Prevention of Cruelty to Animals has prepared its bill upon the supposition that experiments of a nature to cause pain are justifiable if they are performed when the animal has first been rendered wholly insensible to pain, and is destroyed before the effect of the anæsthetic ceases. The bill also provides that the place shall be registered, and that no such experiments shall be made by anyone while lecturing or giving instruction to students in classes or otherwise. The secretary has told us that the society is formed for the prevention of cruelty, and

would not step out of its direction to legalize anything of a contrary nature; yet he says, in reference to a supposed case in which a great good was aimed at, and a small amount of suffering inflicted, that in such a case, even if their own bill had become law, the society would not prosecute. But we have already stated that experiments under complete anæsthesia may be used in the course of a lecture without objection on the score of cruelty; and there might be many experiments made for the purpose of original research in which the injury inflicted would be very small, and the pain would be almost nil, and in which it might be more humane to permit the animal to enjoy life, than to destroy it.

The bill introduced last session into the House of Lords went upon the principle that no experiment should be lawful unless the animal were completely under the influence of an anæsthetic, and the experiment were performed in a place registered by the Secretary of State. Any person might apply to the Secretary of State for a special license to perform vivisections without the use of anæsthetics; such license was to continue for six months, and no longer. On proof of any abuse, the registration of the place might be withdrawn.

The bill introduced into the House of Commons prohibited all experiments causing pain, or of a nature to cause pain, with the following exceptions, viz. :—1st. For the purpose of new scientific discovery, but for no other purpose, an experiment might be made, provided that the animal was placed and kept under complete anæsthesia. 2nd. Where for the purpose of new scientific discovery, and for no other purpose, an experiment was desired to be made in which insensibility could not be produced without necessarily frustrating the object of the experiment, it might be performed by a person holding a license granted by the Secretary of State under certain conditions.

As regards the last bill, if adopted precisely as it was proposed, it would have prevented the use of an experiment for the purpose of demonstration before a medical class in a public institution, however absolute the security for complete anæsthesia :—an object not intended to be arrived at, we presume, by the framers of the bill.

What we should humbly recommend to Your Majesty would be the enactment of a law by which experiments upon living animals, whether for original research or for demonstration, should be placed under the control of the Secretary of State, who should have power to grant licenses to persons, and, when satisfied of the propriety of doing so, to withdraw them. No other persons should be permitted to perform experiments. The holders of licenses should be bound by conditions, and breach of the conditions should entail the liability to forfeiture of the license; the object of the conditions should be to ensure that suffering should never be inflicted in any case in which it could be avoided, and should be reduced to a minimum where it could not be altogether avoided. This should be the general scope of the conditions; but their detailed application should be left to be modified from time

to time by the minister responsible according to the dictates of experience. In the administration of the system generally, the responsible minister would of course be guided by the opinion of advisers of competent knowledge and experience. Dr. Playfair's bill provided a machinery for the purpose, and some arrangements of the kind proposed in that measure would be necessary. But we think it is inexpedient to divide the responsibility of the Secretary of State with that of other persons by statutory enactment, and we recommend that his advisers should be from time to time selected and nominated by himself. Their names should be made known to the profession and the public. It may be found desirable that one of the conditions to be attached to a license should be that the experiments should be performed in some particular place; but this is a detail which may vary with circumstances, and we think it ought not to be stereotyped by statute.

The Secretary of State must have the most complete power of efficient inspection and of obtaining full returns and accurate records of all experiments made. Any place in which experiments are performed must be registered and open to efficient inspection. The appointment of an inspector or inspectors will be necessary, and we have seen that the analogy of the Anatomy Act has been appealed to by many high authorities. It is to be observed that the duties under that Act are of a nature much more mechanical than those which will be required in the present instance. The inspectors must be persons of such character and position as to command the confidence of the public no less than that of men of science.

Abuse of the power conferred by the license must, of course, render the holder liable to its withdrawal; but this will involve great disgrace; and the withdrawal of the license of an eminent man without real cause might be a serious public mischief. We have felt it necessary, therefore, to consider what steps should be taken when the question of such withdrawal may arise. We think that the holder of a license, when he shall receive notice that the Secretary of State intends to withdraw it during the period for which it has been granted, should be at liberty to demand a public inquiry; that this inquiry should be held before one of the Judges of the Supreme Court, with two competent assessors to be appointed by the Secretary of State, the Court having the full power of conducting it as a legal investigation by summoning and swearing witnesses, issuing commissions, and so forth:—that on the result of this inquiry, the Secretary of State should determine whether the license ought to be withdrawn, and when he decides in the negative, should have the power of giving the holder of the license the reasonable costs of his defence.

Magistrates ought to be empowered, on cause shown, to authorise the police to enter and search the premises of persons suspected of performing experiments without a license, and the performance of such experiments without a license should be penal.

the source of supply, an eminent physiologist said (answer 2822), "I know as regards rabbits and guinea pigs, and so forth, that they are drawn from the usual sources, that they are bought in the market;" but when asked specially as to dogs and cats, he replied (answer 2823), I cannot tell you where they come from. There is no proper provision in this country by which one can obtain dogs, even for the most legitimate purposes, and of course I am not informed as to the way in which they are obtained. They are always paid for at a proper price." And another equally eminent witness, who had told us that in some two or three series of experiments on the cholera poison he had used considerably more than 90 cats, that being the number which he used (answer 5747) in one of those series of experiments only—replied to a question touching the source of supply, "they are supplied to me by a man;" and when further asked whether the person referred to obtained them in a legitimate way, he answered, "I make no inquiries" (5734, 5735). It would appear, therefore, that however painless the investigations conducted on dogs and cats may sometimes be, there is a special abuse to which even such painless experiments are liable, of a kind not affecting the experiments on rabbits and other creatures of which there is a sufficient market supply at low prices, namely, that the former furnish a strong motive for an illicit trade, not only degrading in itself, but causing frequently great distress to the owners of the creatures decoyed away. The second reason for this restriction with which our evidence has furnished us, is contained in an answer of Dr. Anthony, the pupil and dissector of Sir Charles Bell, who when asked whether the domestic animals are not liable to that special sensibility or "hyperæsthesia" to which civilized men appear to be so much more subject than barbarous tribes, replied, (answer 2596), "I am inclined to think so, that you have brought both under the 'influence of what you may term civilization.' It is true that the same witness told us, with obvious justice, that even in the same species you would find some creatures of very low, and others of very high sensibility, and that, in his opinion, intelligence might be taken as almost a measure of sensation (2598); but as it will be impossible to draw fine distinctions in such cases, even if the other reasons affecting the question admitted of any such distinctions, and as it is notorious that no class of animals otherwise convenient for experimentation contains so many creatures of high intelligence, and therefore probably of high sensibility, as dogs and cats, it seems to me desirable, in consideration of this special sensibility, to exempt these members of our households from all liability of experimentation. A third reason for this exemption seems to suggest itself from the very nature of our relations to these creatures, which we have trained up in habits of obedience to man and of confidence, in him, so that there is something of the nature of treachery as well as of insensibility to their sufferings, in allowing them to be subjected to

severe pain even in the interests of science. No doubt it is the intention of the measure we have recommended to reduce animal suffering to a minimum in all cases ; indeed I may be allowed to say that the measure proposed will not at all satisfy my own conception of the needs of the case, unless it results in putting an end to all experiments involving not merely torture but anything at all approaching it ; for where the pursuit of scientific truth and common compassion come into collision, it seems to me that the ends of civilization, no less than of morality, require us to be guided by the latter and higher principle. But as there is no mode by which the discretion of the licensee, so long as he continues to hold his license, can be limited, I think we must assume that the subjects of these experiments will continue to be liable to a greater or less degree of suffering ; and that we should try to diminish the evil involved in the infliction of that suffering to its lowest point. And if suffering is to be inflicted at all, with whatever humane economy it is meted out, is it better both as regards the evil, of enduring and the evil of inflicting it, that the humble friends of man, which have been taught to obey and trust him, should not be selected as the victims. I may add that I do not find any trace in our evidence that there is a single one of the important scientific discoveries which have been represented to us as due to, or as finally verified by, experiments of this kind, of which science would have been deprived had any such limitation as this been at the time in force.

I do not include all the domestic animals in this suggestion, for two reasons. In the first place it is not necessary. The animals useful for agriculture or for purposes of food—like horses, oxen, and sheep—are a great deal too valuable, a great deal too well guarded, and for the most part a great deal too large for the purposes of ordinary physiological experiment. And in the next place, in the interest of these classes of animals themselves, it would be undesirable. The only sort of experiments to which they are commonly subjected are pathological experiments, *i.e.*, artificial inoculations with disease, made with the view of discovering some cure or some mitigation for the epidemics which periodically decimate them, like cattle plague or sheep-pox. Such experiments stand on a somewhat different footing from experiments made purely or chiefly in the interest of man himself. They are not very numerous ; wherever they endanger life they are necessarily costly ; and they may result in discoveries of the highest possible benefit to the races of creatures in whose behalf they are made.

I know of but one serious objection to this proposal. It has been urged upon me that by drawing a distinction in favour of certain classes of animals, the legislature would be taking a step in the wrong direction, since all humane people are desirous to see the Cruelty to Animals Act (commonly called Martin's Act), which at present protects domestic animals only, extended to wild animals ; whereas the

adoption of such a restriction as I have suggested would furnish, it is said, a new excuse for leaving wild animals unprotected by law. I cannot see the force of this objection, which, if it were valid, would be a very serious one, since I strongly desire to see the scope of Martin's Act extended so as to include creatures of all kinds. We should not, I think, be the less anxious to guard the less sensitive creatures from torture, because we had put the more sensitive under special safeguards. It seems to me that creatures bound to us by special ties may well and safely be permitted special privileges; indeed, that the tendency of any measure which recognised more explicitly the claims of our family dependents to be especially guarded from anything like hostile treatment, would have a generally humanizing influence on social manners, and improve instead of deteriorating the treatment even of wild animals.

RICHARD HOLT HUTTON. (SEAL.)

NATHANIEL BAKER, *Secretary*,
8th January, 1876.

It is due to Dr. Richardson to publish the following statement made to Mr. Colam in 1862.—“I know of no institution in the kingdom where vivisection is carried on publicly, or even privately, in a systematic manner. 2. Vivisection, when performed in England, is according to my experience, always conducted by professors who have some definite object of investigation in view. 3. I have never seen a vivisection instituted for the mere purpose of instructing students, and I believe the practice would be contrary to the feelings of professors and of students, equally: further I have never seen a student make a vivisection for his own inquiries, and I believe that no school in the kingdom would allow its students to conduct vivisections for such objects. 4. Vivisections in England are conducted in the present day on animals rendered unconscious of pain by some anæsthetic. Chloroform and the fume of the ‘*lycoperden gigantium*’ are so effective and so easily applied that the physiological operator thinks of proceeding without one or other of them, as rarely as does the surgeon. Indeed, the employment of anæsthetics in operative physiology is now recognised, not only as a humane practice, but as securing better experimental results. 5. Vivisection in this country is comparatively rare as a practice; none resort to it but men far advanced in physiological learning, and they only for the solution of important questions. 6. I believe the day will come when physiological inquiry will be so far complete that the practice of vivisection will pass away altogether. As yet, however, there are problems of profoundest interest relating to some of the animal functions, for the solution of which vivisection, unfortunately, is still required. Hence the practice remains as a necessity; but, as carried out in this country it is certainly performed with the least possible infliction of pain.—B. W. RICHARDSON, M.A., M.D.”

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The Committee of the R.S.P.C.A. are responsible only for directing its Secretary to prepare this Volume. The imperfections are mine.—JOHN COLAM.

1 Names of Witnesses,	2 Are experiments necessary for original re- search?	3 Are experiments for original research per- formed by yourself? *	4 Are experiments for original research necessarily painful to animals?	When applica- tion of ethic fact in or are tain an
WATSON,	Yes	No	Seldom need be	P
BURROWS,	Yes	No	Seldom need be	P
PAGET,	Yes	No	Seldom need be	P
SHARPEY,	Yes	No	Seldom need be	P
HUMPHRY,	Yes	Seldom need be	P
PRITCHARD,	Yes	P
ACLAND,	Yes	Seldom need be	P
FERGUSON,	Yes	No
TAYLOR,	Yes	Yes	Must sometimes be +
ROLLESTON,	Yes	Yes	Seldom need be	P
SIMON,	Yes	Yes	Seldom need be	P
COLAM,	Must sometimes be +
WALKER,	Yes	No	No
CAPE,	Yes	No
MACILWAIN,	No	No
HAUGHTON,	Yes	Yes	Seldom need be	P
GARROD,	Yes	Yes	Seldom need be	P
PAVY,	Yes	Yes	Seldom need be	P
PYE-SMITH,	Yes	Yes	Seldom need be	P
SANDERSON,	Yes	Yes	Seldom need be	P
FOSTER,	Yes	Yes	Seldom need be	P
ANTHONY,	Yes	Seldom need be	P
RUTHERFORD,	Yes	Yes	Seldom need be	P
TURNER,	Yes	Yes	Seldom need be	P
BROWNE,	Yes	Yes	Seldom need be	P
FERRIER,	Yes	Yes	Seldom need be	P
HOGGAN,	Yes	No	Many must be	D
KLEIN,	Yes	Yes	Pain immaterial	P
SCHAEFER,	Yes	Yes	Seldom need be	P
MCKENDRICK,	Yes	Yes	Seldom need be	P
SAWYER,*
LISTER,	Yes	Yes	Seldom need be	P
MCDONNELL,	Yes	Yes	Seldom need be	P
HAYDEN,	Yes
CLELAND,	Yes	Seldom need be	P
DARWIN,	Yes	No	Seldom need be	P
SIBSON,	Yes	Yes	Seldom need be	P
PURSER,	Yes	Seldom need be	F
MILLS,	No	Generally painless	P
SCOTT,	No	No
LEGG,	Yes	Yes	Seldom need be	F
GAMGEE,	Yes	Yes	Seldom need be	F
ALLMAN,	Yes	Seldom need be	F
GULL,	Yes	Seldom need be	F
CARPENTER,	Yes	Seldom need be	F
BRUNTON,	Yes	Yes	Must sometimes be	F
SINCLAIR,	Yes	Yes	Seldom need be	F
HANDYSIDE,	Yes	Seldom need be	F
WILLIAMS,	Yes §	F
CRISP,	Yes
HOLT,
LEWES,	Yes	Yes	Seldom need be	F
JESSE,

5 properly do anas- ensure per- sensitivity,* they uncer- delusive?	6 Does Curare in- crease, decrease, or prevent pain, or is its effect on the sensory nerves doubtful?	7 Are deductions re- liable based on experiments of poisons and other drugs supposed to be poisons?*	8 Are experiments on living animals necessary for teach- ing medical students?*	9 Are demonstrations living animals perform himself before med students in classes
effect	Doubtful	Unreliable	No	
effect	Decreases	Reliable	No	
effect		Reliable	Yes (painless)	
effect	Doubtful		Yes (painful)	
effect	Doubtful	Unreliable		
effect	Doubtful		No	No
effect	Doubtful		Yes	Yes (painless)
.....	Doubtful		No	No
.....	Doubtful	Reliable	No	No
effect	Doubtful	Reliable	Yes (painless)	Rarely
effect			Yes (painless)	
			No	
	Doubtful		No	
			No	
		Unreliable	No	
effect		Unreliable	No	No
effect			No	No
effect			Yes (painless)	Yes (painless)
effect			Yes (painless)	Yes (painless)
effect	Doubtful	Reliable	Yes (painless)	Yes (painless)
effect	Doubtful	Reliable	Yes (painless)	Yes (painless)
effect			No	No
effect	Decreases	Doubtful†	Yes	Yes
effect		Reliable	Yes	No
effect				
effect			Yes	Yes
elusive	Increases*	Unreliable	Yes (painful)	
effect	Decreases		Yes	Yes
effect	Doubtful		Yes	Yes
effect			Yes (painless)	Yes (painless)
effect			Yes (painless)	
effect			Yes (painless)	Rarely
			No	Yes (painless)
effect			Yes	No
effect			Yes (painless)	Yes (painless)
effect	Decreases		Yes (painless)	
effect			Yes (painful)	
effect			No	No
	Doubtful		No	No
effect		Reliable		No
effect	Doubtful		Yes (painless)	Yes (painless)
effect			Yes (painless)	No
effect			Yes (painful)	
effect		Reliable	Yes (painless)	
effect	Decreases	Reliable	Yes (painless)	Yes (painless)
effect				No
effect		Reliable	No	Yes (painless)
effect			Yes (painless)	Yes (painless)
effect	Prevents		No	
			Yes	
			No	

FABULAR DIGEST OF EVIDENCE.

	10	11	12	
on liby al	Are students per- mitted to practice vivisection in his medical school?	Are instances known to you of medical students experiment- ing on living animals secretly?	Are continental experiments generally more painful than those performed in this country?	Opinions on legislation and it useful Analysis small com- answer has ference are of minutes do not
		No	Yes	It should restrict
		No	Yes	It should restrict
	No	No	Yes	Doubts its utility
Painless ones	No	No		Doubts its utility
No	No	No		Public opinion is
No	No	No	Yes	Objects to have no others in
Believes so	No	No		It should restrain
No	No	No	Yes	It should restrict
No	No	One	Yes	Objects—Cruelty
			Yes	It should absolutely
			Yes	It should restrict
				It should restrict
No				Being fallacious
No		No	Yes	It should restrict
No		No	Yes	Competent perso-
No		No	Yes	Does not object
No		No	Yes	Doubts its utility
No		No	Yes	Doubts its utility
No		No	Yes	Doubts its utility
	Believes they do		Yes	It should restrict
No	No		Yes	Doubts its utility
No	No			Objects and doubt
				Doubts its utility
	Believes they do		No	Objects to interfere
Yes	No		Yes	Should license
	No		No	Uncalled for
	One			isifies pain.
				Doubts its utility
				had better
No	No			Uncalled for
				It should restrict
				it, however.
				Cruelty Statute
			Yes	It should restrict
No	No			Doubts its utility
Yes (bleeding)*	Three or four			Uncalled for
	Believes they do			ation of its
				until it is
No	No			Should abolish e
				phylline) is
No	No		Yes	It should restrict
				It should restrict
				Objects to interfere
				It should restrict
No	No			students who
No	No			It should restrict
Yes (bleeding)*	No			It should restrict
				it gives no
				Cannot be suppr
				Should abolish es.
	No		Yes	Objects to interfere
				Should abolish e

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